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MEMORY

A CONTRIBUTION TO EXPERIMENTAL PSYCHOLOGY

BY

HERMANN EBBINGHAUS

PRIVAT DOCENT IN PHILOSOPHY AT THE UNIVERSITY OF BERLIN

(1885)

"*De subjecto vetustissimo*
novissimam promovemus scientiam"

TRANSLATED BY

HENRY A. RUGER, PH.D.

ASSISTANT PROFESSOR OF EDUCATIONAL PSYCHOLOGY, TEACHERS COLLEGE, COLUMBIA UNIVERSITY

AND

CLARA E. BUSSENIUS

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TRANSLATORS' INTRODUCTION

The publication by Ebbinghaus of the results of his experimental investigation of memory (1885) marks the application of precise scientific method to the study of the "higher mental processes." By his invention of nonsense syllables as the material to be thus employed Ebbinghaus signalised the growing independence of experimental psychology from physics and physiology. For educational psychology his work is of especial importance because the field in which he worked was that of the ideational processes and because the problems which he attacked were functional and dynamic. The problem of the most efficient distribution of repetitions in committing material to memory may be taken to illustrate the identity in the nature of the questions investigated by him and those of especial interest to us to-day. Despite the fact that his experiments were performed only on himself and that the numerical results obtained are consequently limited in significance, his work stands as an embodiment of the essentials of scientific method. On account of its historical importance and also because of its intrinsic relation to present day problems and methods Ebbinghaus's investigation should be known as directly as possible by all serious students of psychology. To facilitate this acquaintance is the purpose of this translation.

The translators wish to acknowledge their indebtedness to Professors Edward L. Thorndike, Robert S. Woodworth, and E. W. Bagster-Collins of Columbia University, to Professor Walter Dill Scott of Northwestern University and to Mrs. H. A Ruger for assistance in revising manuscript and proof.

AUTHOR'S PREFACE

In the realm of mental phenomena, experiment and measurement have hitherto been chiefly limited in application to sense perception and to the time relations of mental processes. By mean. of the following investigations we have tried to go a step farther into the workings of the mind and to submit to an experimental and quantitative treatment the manifestations of memory. The term, memory, is to be taken here in its broadest sense, including Learning, Retention, Association and Reproduction.

The principal objections which, as a matter of course, rise against the possibility of such a treatment are discussed in detail in the text and in part have been made objects of investigation. I may therefore ask those who are not already convinced *a priori* of the impossibility of such an attempt to postpone their decision about its practicability.

The author will be pardoned the publication of preliminary results in view of the difficulty of the subject investigated and the time-consuming character of the tests. Justice demands that the many defects due to incompleteness shall not be raised as objections against such results. The tests were all made upon myself and have primarily only individual significance. Naturally they will not reflect exclusively mere idiosyncrasies of my mental organisation; if the absolute values found are throughout only individual, yet many a relation of general validity will be found in the relation of these numbers to each other or in the relations of the relations. But where this is the case and where it is not, I can hope to decide only after finishing the further and comparative experiments with which I am occupied.

TABLE OF CONTENTS

MEMORY

CHAPTER I

OUR KNOWLEDGE CONCERNING MEMORY

Section 1. Memory in its Effects

The language of life as well as of science in attributing a memory to the mind attempts to point out the facts and their interpretation somewhat as follows:

Mental states of every kind,—sensations, feelings, ideas,—which were at one time present in consciousness and then have disappeared from it, have not with their disappearance absolutely ceased to exist. Although the inwardly-turned look may no longer be able to find them, nevertheless they have not been utterly destroyed and annulled, but in a certain manner they continue to exist, stored up, so to speak, in the memory. We cannot, of course, directly observe their present existence, but it is revealed by the effects which come to our knowledge with a certainty like that with which we infer the existence of the stars below the horizon. These effects are of different kinds.

In a first group of cases we can call back into consciousness by an exertion of the will directed to this purpose the seemingly lost states (or, indeed, in case these consisted in immediate sense-perceptions, we can recall their true memory images): that is, we can reproduce them *voluntarily*. During attempts of this sort,—that is, attempts to recollect—all sorts of images toward which our aim was not directed, accompany the desired images to the light of consciousness. Often, indeed, the latter entirely miss the goal, but as a general thing among the representations is found the one which we sought, and it is immediately recognised as something formerly experienced. It would be absurd to suppose that our will has created it anew and, as it were, out of nothing; it must have been present somehow

or somewhere. The will, so to speak, has only discovered it and brought it to us again.

In a second group of cases this survival is even more striking. Often, even after years, mental states once present in consciousness return to it with apparent spontaneity and without any act of the will; that is, they are reproduced *involuntarily*. Here, also, in the majority of cases we at once recognise the returned mental state as one that has already been experienced; that is, we remember it. Under certain conditions, however, this accompanying consciousness is lacking, and we know only indirectly that the "now" must be identical with the "then"; yet we receive in this way a no less valid proof for its existence during the intervening time. As more exact observation teaches us, the occurrence of these involuntary reproductions is not an entirely random and accidental one. On the contrary they are brought about through the instrumentality of other, immediately present mental images. Moreover they occur in certain regular ways which in general terms are described under the so-called 'laws of association.'

Finally there is a third and large group to be reckoned with here. The vanished mental states give indubitable proof of their continuing existence even if they themselves do not return to consciousness at all, or at least not exactly at the given time. Employment of a certain range of thought facilitates under certain conditions the employment of a similar range of thought, even if the former does not come before the mind directly either in its methods or in its results. The boundless domain of the effect of accumulated experiences belongs here. This effect results from the frequent conscious occurrence of any condition or process, and consists in facilitating the occurrence and progress of similar processes. This effect is not fettered by the condition that the factors constituting the experience shall return *in toto* to consciousness. This may incidentally be the case with a part of them; it must not happen to a too great extent and with too great clearness, otherwise the course of the present process will immediately be disturbed. Most of these experiences remain concealed from consciousness and yet produce an effect which is significant and which authenticates their previous existence.

Section 2. Memory in its Dependence

Along with this bare knowledge of the existence of memory and its *effects*, there is abundant knowledge concerning the *conditions* upon which depend the vitality of that inner survival as well as the fidelity and promptness of the reproduction.

How differently do different *individuals* behave in this respect! One retains and reproduces well; another, poorly. And not only does this comparison hold good when different individuals are compared with each other, but also when different phases of the existence of the same individual are compared: morning and evening, youth and old age, find him different in this respect.

Differences in the *content* of the thing to be reproduced are of great influence. Melodies may become a source of torment by the undesired persistency of their return. Forms and colors are not so importunate; and if they do return, it is with noticeable loss of clearness and certainty. The musician writes for the orchestra what his inner voice sings to him; the painter rarely relies without disadvantage solely upon the images which his inner eye presents to him; nature gives him his forms, study governs his combinations of them. It is with something of a struggle that past states of feeling are realized; when realized, and this is often only through the instrumentality of the movements which accompanied them, they are but pale shadows of themselves. Emotionally true singing is rarer than technically correct singing.

If the two foregoing points of view are taken together—differences in individuals and differences in content—an endless number of differences come to light. One individual overflows with poetical reminiscences, another directs symphonies from memory, while numbers and formulae, which come to a third without effort, slip away from the other two as from a polished stone.

Very great is the dependence of retention and reproduction upon the intensity of the *attention* and *interest* which were attached to the mental states the first time they were present. The burnt child shuns the fire, and the dog which has been beaten runs from the whip, after a single vivid experience. People in whom we are interested we may see daily

and yet not be able to recall the color of their hair or of their eyes.

Under ordinary circumstances, indeed, frequent repetitions are indispensable in order to make possible the reproduction of a given content. Vocabularies, discourses, and poems of any length cannot be learned by a single repetition even with the greatest concentration of attention on the part of an individual of very great ability. By a sufficient number of repetitions their final mastery is ensured, and by additional later reproductions gain in assurance and ease is secured.

Left to itself every mental content gradually loses its capacity for being revived, or at least suffers loss in this regard under the influence of time. Facts crammed at examination time soon vanish, if they were not sufficiently grounded by other study and later subjected to a sufficient review. But even a thing so early and deeply founded as one's mother tongue is noticeably impaired if not used for several years.

Section 3. Deficiencies in our Knowledge concerning Memory

The foregoing sketch of our knowledge concerning memory makes no claim to completeness. To it might be added such a series of propositions known to psychology as the following: "He who learns quickly also forgets quickly," "Relatively long series of ideas are retained better than relatively short ones," "Old people forget most quickly the things they learned last," and the like. Psychology is wont to make the picture rich with anecdote and illustration. But—and this is the main point—even if we particularise our knowledge by a most extended use of illustrative material, everything that we can say retains the indefinite, general, and comparative character of the propositions quoted above. Our information comes almost exclusively from the observation of extreme and especially striking cases. We are able to describe these quite correctly in a general way and in vague expressions of more or less. We suppose, again quite correctly, that the same influences exert themselves, although in a less degree, in the case of the inconspicuous, but a thousand-fold more frequent, daily activities of memory. But if our curiosity carries us further and we crave more specific and detailed information concerning these dependencies and inter-

dependencies, both those already mentioned and others,—if we put questions, so to speak, concerning their inner structure—our answer is silence. How does the disappearance of the ability to reproduce, forgetfulness, depend upon the length of time during which no repetitions have taken place? What proportion does the increase in the certainty of reproduction bear to the number of repetitions? How do these relations vary with the greater or less intensity of the interest in the thing to be reproduced? These and similar questions no one can answer.

This inability does not arise from a chance neglect of investigation of these relations. We cannot say that tomorrow, or whenever we wish to take time, we can investigate these problems. On the contrary this inability is inherent in the nature of the questions themselves. Although the conceptions in question—namely, degrees of forgetfulness, of certainty and interest—are quite correct, we have no means for establishing such degrees in our experience except at the extremes, and even then we cannot accurately limit those extremes. We feel therefore that we are not at all in a condition to undertake the investigation. We form certain conceptions during striking experiences, but we cannot find any realisation of them in the similar but less striking experiences of everyday life. *Vice versa* there are probably many conceptions which we have not as yet formed which would be serviceable and indispensable for a clear understanding of the facts, and their theoretical mastery.

The amount of detailed information which an individual has at his command and his theoretical elaborations of the same are mutually dependent; they grow in and through each other. It is because of the indefinite and little specialised character of our knowledge that the theories concerning the processes of memory, reproduction, and association have been up to the present time of so little value for a proper comprehension of those processes. For example, to express our ideas concerning their physical basis we use different metaphors—stored up ideas, engraved images, well-beaten paths. There is only one thing certain about these figures of speech and that is that they are not suitable.

Of course the existence of all these deficiencies has its perfectly sufficient basis in the extraordinary difficulty and complexity of the matter. It remains to be proved whether, in spite

of the clearest insight into the inadequacy of our knowledge, we shall ever make any actual progress. Perhaps we shall always have to be resigned to this. But a somewhat greater accessibility than has so far been realised in this field cannot be denied to it, as I hope to prove presently. If by any chance a way to a deeper penetration into this matter should present itself, surely, considering the significance of memory for all mental phenomena, it should be our wish to enter that path at once. For at the very worst we should prefer to see resignation arise from the failure of earnest investigations rather than from persistent, helpless astonishment in the face of their difficulties.

CHAPTER II

THE POSSIBILITY OF ENLARGING OUR KNOWL EDGE OF MEMORY

Section 4. The Method of Natural Science

The method of obtaining exact measurements—*i.e.*, num ically exact ones—of the inner structure of causal relations by virtue of its nature, of general validity. This method, inde has been so exclusively used and so fully worked out the natural sciences that, as a rule, it is defined as somethi peculiar to them, as *the* method of natural science. To repe however, its logical nature makes it generally applicable to spheres of existence and phenomena. Moreover, the possibili of defining accurately and exactly the actual behavior of a process whatever, and thereby of giving a reliable basis for t direct comprehension of its connections depends above all up the possibility of applying this method.

We all know of what this method consists: an attempt made to keep constant the mass of conditions which ha proven themselves causally connected with a certain result; o of these conditions is isolated from the rest and varied in way that can be numerically described; then the accompanyi change on the side of the effect is ascertained by measurem or computation.

Two fundamental and insurmountable difficulties, seem, ho ever, to oppose a transfer of this method to the investigation the causal relations of mental events in general and of those memory in particular. In the first place, how are we to ke even approximately constant the bewildering mass of cau conditions which, in so far as they are of mental nature, alm completely elude our control, and which, moreover, are subj to endless and incessant change? In the second place, by wh possible means are we to measure numerically the mental pr cesses which flit by so quickly and which on introspection are

hard to analyse? I shall first discuss the second difficulty in connection, of course, with memory, since that is our present concern.

Section 5. Introduction of Numerical Measurements for Memory Contents

If we consider once more the conditions of retention and reproduction mentioned above (§ 2), but now with regard to the possibility of computation, we shall see that with two of them, at least, a numerical determination and a numerical variation are possible. The different times which elapse between the first production and the reproduction of a series of ideas can be measured and the repetitions necessary to make these series reproducible can be counted. At first sight, however, there seems to be nothing similar to this on the side of the effects. Here there is only one alternative, a reproduction is either possible or it is not possible. It takes place or it does not take place. Of course we take for granted that it may approach, under different conditions, more or less near to actual occurrence so that in its subliminal existence the series possesses graded differences. But as long as we limit our observations to that which, either by chance or at the call of our will, comes out from this inner realm, all these differences are for us equally non-existent.

By somewhat less dependence upon introspection we can, however, by indirect means force these differences into the open. A poem is learned by heart and then not again repeated. We will suppose that after a half year it has been forgotten: no effort of recollection is able to call it back again into consciousness. At best only isolated fragments return. Suppose that the poem is again learned by heart. It then becomes evident that, although to all appearances totally forgotten, it still in a certain sense exists and in a way to be effective. The second learning requires noticeably less time or a noticeably smaller number of repetitions than the first. It also requires less time or repetitions than would now be necessary to learn a similar poem of the same length. In this difference in time and number of repetitions we have evidently obtained a certain measure for that inner energy which a half year after the first learning still dwells in that orderly complex of ideas which make up the

poem. After a shorter time we should expect to find the difference greater; after a longer time we should expect to find it less. If the first committing to memory is a very careful and long continued one, the difference will be greater than if it is desultory and soon abandoned.

In short, we have without doubt in these differences numerical expressions for the difference between these subliminally persistent series of ideas, differences which otherwise we would have to take for granted and would not be able to demonstrate by direct observation. Therewith we have gained possession of something that is at least like that which we are seeking in our attempt to get a foothold for the application of the method of the natural sciences: namely, phenomena on the side of the effects which are clearly ascertainable, which vary in accordance with the variation of conditions, and which are capable of numerical determination. Whether we possess in them correct measures for these inner differences, and whether we can achieve through them correct conceptions as to the causal relations into which this hidden mental life enters—these questions cannot be answered *a priori*. Chemistry is just as little able to determine *a priori* whether it is the electrical phenomena, or the thermal, or some other accompaniment of the process of chemical union, which gives it its correct measure of the effective forces of chemical affinity. There is only one way to do this, and that is to see whether it is possible to obtain, on the presupposition of the correctness of such an hypothesis, well classified, uncontradictory results, and correct anticipations of the future.

Instead of the simple phenomenon—occurrence or non-occurrence of a reproduction—which admits of no numerical distinction, I intend therefore to consider from the experimental standpoint a more complicated process as the effect, and I shall observe and measure its changes as the conditions are varied. By this I mean the artificial bringing about by an appropriate number of repetitions of a reproduction which would not occur of its own accord.

But in order to realise this experimentally, two conditions at least must be fulfilled.

In the first place, it must be possible to define with some certainty the moment when the goal is reached—*i.e.*, when the

process of learning by heart is completed. For if the process of learning by heart is sometimes carried past that moment and sometimes broken off before it, then part of the differences found under the varying circumstances would be due to this inequality, and it would be incorrect to attribute it solely to inner differences in the series of ideas. Consequently among the different reproductions of, say, a poem, occurring during the process of its memorisation, the experimenter must single out one as especially characteristic, and be able to find it again with practical accuracy.

In the second place the presupposition must be allowed that the number of repetitions by means of which, the other conditions being unchanged, this characteristic reproduction is brought about would be every time the same. For if this number, under conditions otherwise equivalent, is now this and now that, the differences arising from varied conditions lose, of course, all significance for the critical evaluation of those varying conditions.

Now, as far as the first condition is concerned, it is easily fulfilled wherever you have what may properly be called learning by heart, as in the case of poems, series of words, tone-sequences, and the like. Here, in general, as the number of repetitions increases, reproduction is at first fragmentary and halting; then it gains in certainty; and finally takes place smoothly and without error. The first reproduction in which this last result appears can not only be singled out as especially characteristic, but can also be practically recognised. For convenience I will designate this briefly as the *first possible reproduction.*

The question now is:—Does this fulfill the second condition mentioned above? Is the number of repetitions necessary to bring about this reproduction always the same, the other conditions being equivalent?

However, in this form, the question will be justly rejected because it forces upon us, as if it were an evident supposition, the real point in question, the very heart of the matter, and admits of none but a misleading answer. Anyone will be ready to admit without hesitation that this relation of dependence will be the same if perfect equality of experimental conditions is maintained. The much invoked freedom of the will, at least,

has hardly ever been misunderstood by anybody so far as to come in here. But this theoretical constancy is of little value: How shall I find it when the circumstances under which I am actually forced to make my observations are never the same? So I must rather ask:—Can I bring under my control the inevitably and ever fluctuating circumstances and equalise them to such an extent that the constancy presumably existent in the causal relations in question becomes visible and palpable to me?

Thus the discussion of the one difficulty which opposes an exact examination of the causal relations in the mental sphere has led us of itself to the other (§ 4). A numerical determination of the interdependent changes of cause and effect appears indeed possible if only we can realise the necessary uniformity of the significant conditions in the repetition of our experiments.

Section 6. The Possibility of Maintaining the Constancy of Conditions Requisite for Research

He who considers the complicated processes of the higher mental life or who is occupied with the still more complicated phenomena of the state and of society will in general be inclined to deny the possibility of keeping constant the conditions for psychological experimentation. Nothing is more familiar to us than the capriciousness of mental life which brings to nought all foresight and calculation. Factors which are to the highest degree determinative and to the same extent changeable, such as mental vigor, interest in the subject, concentration of attention, changes in the course of thought which have been brought about by sudden fancies and resolves—all these are either not at all under our control or are so only to an unsatisfactory extent.

However, care must be taken not to ascribe too much weight to these views, correct in themselves, when dealing with fields other than those of the processes by the observation of which these views were obtained. All such unruly factors are of the greatest importance for higher mental processes which occur only by an especially favorable concurrence of circumstances. The more lowly, commonplace, and constantly occurring processes are not in the least withdrawn from their influence, but we have it for the most part in our power, when it is a matter of consequence, to make this influence only slightly disturbing. Sensorial perception, for example, certainly occurs with greater

or less accuracy according to the degree of interest; it is constantly given other directions by the change of external stimuli and by ideas. But, in spite of that, we are on the whole sufficiently able to see a house just when we want to see it and to receive practically the same picture of it ten times in succession in case no objective change has occurred.

There is nothing *a priori* absurd in the assumption that ordinary retention and reproduction, which, according to general agreement, is ranked next to sensorial perception, should also behave like it in this respect. Whether this is actually the case or not, however, I say now as I said before, cannot be decided in advance. Our present knowledge is much too fragmentary, too general, too largely obtained from the extraordinary to enable us to reach a decision on this point by its aid; that must be reserved for experiments especially adapted to that purpose. We must try in experimental fashion to keep as constant as possible those circumstances whose influence on retention and reproduction is known or suspected, and then ascertain whether that is sufficient. The material must be so chosen that decided differences of interest are, at least to all appearances, excluded; equality of attention may be promoted by preventing external disturbances; sudden fancies are not subject to control, but, on the whole, their disturbing effect is limited to the moment, and will be of comparatively little account if the time of the experiment is extended, etc.

When, however, we have actually obtained in such manner the greatest possible constancy of conditions attainable by us, how are we to know whether this is sufficient for our purpose? When are the circumstances, which will certainly offer differences enough to keen observation, sufficiently constant? The answer may be made:—When upon repetition of the experiment the results remain constant. The latter statement seems simple enough to be self-evident, but on closer approach to the matter still another difficulty is encountered.

Section 7. Constant Averages

When shall the results obtained from repeated experiments under circumstances as much alike as possible pass for constant or sufficiently constant? Is it when one result has the same

value as the other or at least deviates so little from it that the difference in proportion to its own quantity and for our purposes is of no account?

Evidently not. That would be asking too much, and is not necessarily obtained even by the natural sciences. Then, perhaps it is when the averages from larger groups of experiments exhibit the characteristics mentioned above?

Again evidently not. That would be asking too little. For, if observation of processes that resemble each other from any point of view are thrown together in sufficiently large numbers, fairly constant mean values are almost everywhere obtained which, nevertheless, possess little or no importance for the purposes which we have here. The exact distance of two signal poles, the position of a star at a certain hour, the expansion of a metal for a certain increase of temperature, all the numerous coefficients and other constants of physics and chemistry are given us as average values which only approximate to a high degree of constancy. On the other hand the number of suicides in a certain month, the average length of life in a given place, the number of teams and pedestrians per day at a certain street corner, and the like, are also noticeably constant, each being an average from large groups of observations. But both kinds of numbers, which I shall temporarily denote as *constants of natural science* and *statistical constants,* are, as everybody knows, constant from different causes and with entirely different significance for the knowledge of causal relations.

These differences can be formulated as follows:—

In the case of the constants of the natural sciences each individual effect is produced by a combination of causes exactly alike. The individual values come out somewhat differently because a certain number of those causes do not always join the combination with exactly the same values (*e. g.,* there are little errors in the adjustment and reading of the instruments, irregularities in the texture or composition of the material examined or employed, etc.). However, experience teaches us that this fluctuation of separate causes does not occur absolutely irregularly but that as a rule it runs through or, rather, tries out limited and comparatively small circles of values symmetrically distributed around a central value. If several cases a brought together the effects of the separate deviations must m

and more compensate each other and thereby be swallowed up in the central value around which they occur. And the final result of combining the values will be approximately the same as if the actually changeable causes had remained the same not only conceptually but also numerically. Thus, the average value is in these cases the adequate numerical representative of a conceptually definite and well limited system of causal connections; if one part of the system is varied, the accompanying changes of the average value again give the correct measure for the effect of those deviations on the total complex.

On the other hand, no matter from what point of view statistical constants may be considered it cannot be said of them that each separate value has resulted from the combination of causes which by themselves had fluctuated within tolerably narrow limits and in symmetrical fashion. The separate effects arise, rather, from an oftimes inextricable multiplicity of causal combinations of very different sorts, which, to be sure, may share numerous factors with each other, but which, taken as a whole, have no conceivable community and actually correspond only in some one characteristic of the effects. That the value of the separate factors must be very different is, so to say, self evident. That, nevertheless, approximately constant values appear even here by the combining of large groups—this fact we may make intelligible by saying that in equal and tolerably large intervals of time or extents of space the separate causal combinations will be realised with approximately equal frequency; we do this without doing more than to acknowledge as extant a peculiar and marvellous arrangement of nature. Accordingly these constant mean values represent no definite and separate causal systems but combinations of such which are by no means of themselves transparent. Therefore their changes upon variation of conditions afford no genuine measure of the effects of these variations but only indications of them. They are of no direct value for the setting up of numerically exact relations of dependence but they are preparatory to this.

Let us now turn back to the question raised at the beginning of this section. When may we consider that this equality of conditions which we have striven to realise experimentally has been attained? The answer runs as follows: When the average values of several observations are approximately constant and

when at the same time we may assume that the separate cases belong to the same causal system, whose elements, however, are not limited to exclusively constant values, but may run through small circles of numerical values symmetrical around a middle value.

Section 8. The Law of Errors

Our question, however, is not answered conclusively by the statement just made. Suppose we had in some way found satisfactorily constant mean values for some psychical process, how would we go about it to learn whether we might or might not assume a homogeneous causal condition, necessary for their further utilisation? The physical scientist generally knows beforehand that he will have to deal with a single causal combination, the statistician knows that he has to deal with a mass of them, ever inextricable despite all analysis. Both know this from the elementary knowledge they already possess of the nature of the processes before they proceed with the more detailed investigations. Just as, a moment ago, the present knowledge of psychology appeared to us too vague and unreliable to be depended upon for decision about the possibility of constant experimental conditions; so now it may prove insufficient to determine satisfactorily whether in a given case we have to deal with a homogeneous causal combination or a manifold of them which chance to operate together. The question is, therefore, whether we may throw light on the nature of the causation of the results we obtain under conditions as uniform as possible by the help of some other criterion.

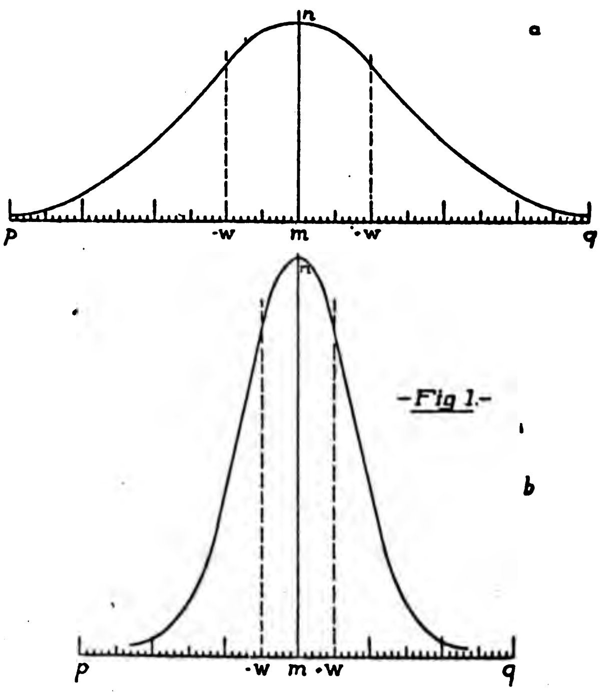
The answer must be: This cannot be done with absolute certainty, but can, nevertheless, be done with great probability. Thus, a start has been made from presuppositions as similar as possible to those by which physical constants have been obtained and the consequences which flow from them have been investigated. This has been done for the distribution of the single values about the resulting central value and quite independently of the actual concrete characteristics of the causes. Repeated comparisons of these calculated values with actual observations have shown that the similarity of the suppositions is indeed great enough to lead to an agreement of the results. The outcome of these speculations closely approximates to reality. It consists

in this,—that the grouping of a large number of separate values that have arisen from causes of the same kind and with the modifications repeatedly mentioned, may be correctly represented by a mathematical formula, the so-called Law of Errors. This is especially characterised by the fact that it contains but one unknown quantity. This unknown quantity measures the relative compactness of the distribution of the separate values around their central tendency. It therefore changes according to the kind of observation and is determined by calculation from the separate values.

NOTE. For further information concerning this formula, which is not here our concern, I must refer to the text-books on the calculation of probabilities and on the theory of errors. For readers unfamiliar with the latter a graphic explanation will be more comprehensible than a statement and discussion of the formula. Imagine a certain observation to be repeated 1,000 times. Each observation as such is represented by a space of one square millimeter, and its numerical value, or rather its deviation from the central value of the whole 1,000 observations, by its position on the horizontal line *p q* of the adjoining Figure I.

For every observation which exactly corresponds with the central value one square millimeter is laid off on the vertical line *m n*. For each observed value which deviates by one unit from the central value upward one square millimeter is laid off on a vertical line to right of *m n* and distant one millimeter from it, etc. For every observed value which deviates by *x* units above (or below) the central value, one sq. mm. is placed on a vertical line distant from *m n* by *x* mms., to the right (or left, for values below the central value). When all the observations are arranged in this way the outer contour of the figure may be so compacted that the projecting corners of the separate squares are transformed into a symmetrical curve. If now the separate measures are of such a sort that their central value may be considered as a constant as conceived by physical science, the form of the resulting curve is of the kind marked *a* and *b* in Fig. I. If the middle value is a statistical constant, the curve may have any sort of a form. (The curves *a* and *b* with the lines *p q* include in each case an area of 1,000 sq. mms. This is strictly the case only with indefinite prolongation of the curves and the lines *p q*, but these lines and curves finally approach each other so closely that where the drawing breaks off only two or three sq. mms. at each end of the curve are missing from the full number.) Whether, for a certain group of observations, the curve has a

more steep or more flat form depends on the nature of thos observations. The more exact they are, the more will they pil up around the central value; and the more infrequent the larg deviations, the steeper will the curve be and *vice versa.* Fo the rest the law of formation of the curve is always the same. Therefore, if a person, in the case of any specific combinatio of observations, obtains any measure of the compactness o distribution of the observations, he can survey the grouping o the whole mass. He could state, for instance, how often a



-Fig 1.-

deviation of a certain value occurs and how many deviation fall between certain limits. Or—as I shall show in what follow —he may state what amount of variation includes between itsel and the central value a certain per cent of all the observed values The lines $+w$ and $-w$ of our figure, for instance, cut ou exactly the central half of the total space representing the obser vations. But in the case of the more exact observations of 1 b they are only one half as far from *m n* as in 1 *a*. So the state ment of their relative distances gives also a measure of th accuracy of the observations.

Therefore, it may be said: wherever a group of effects may be considered as having originated each time from the same causal combination, which was subject each time only to so-called accidental disturbances, then these values arrange themselves in accordance with the "law of errors."

However, the reverse of this proposition is not necessarily true, namely, that wherever a distribution of values occurs according to the law of errors the inference may be drawn that this kind of causation has been at work. Why should nature not occasionally be able to produce an analogous grouping in a more complicated way? In reality this seems only an extremely rare occurrence. For among all the groups of numbers which in statistics are usually condensed into mean values not one has as yet been found which originated without question from a number of causal systems and also exhibited the arrangement summarised by the "law of errors."[1]

Accordingly, this law may be used as a criterion, not an absolutely safe one to be sure, but still a highly probable one, by means of which to judge whether the approximately constant mean values that may be obtained by any proceeding may be employed experimentally as genuine constants of science. The Law of Errors does not furnish sufficient conditions for such a use but it does furnish one of the necessary ones. The final explanation must depend upon the outcome of investigations to the very foundations of which it furnishes a certain security. That is why I applied the measure offered by it to answer our still unanswered question: If the conditions are kept as much alike as is possible, is the average number of repetitions, which is necessary for learning similar series to the point of first possible reproduction, a constant mean value in the natural science sense? And I may anticipate by saying that in the case investigated the answer has come out in the affirmative.

[1] The numbers representing the births of boys and girls respectively, as derived from the total number of births, are said to group themselves in very close correspondence with the law of errors. But in this case it is for this very reason probable that they arise from a homogeneous combination of physiological causes aiming so to speak at the creation of a well determined relation. (See Lexis, Zur Theorie der Massenerscheinungen in der menschlichen Gesellschaft, p. 64 and elsewhere.)

Section 9. Resumé

Two fundamental difficulties arise in the way of the application of the so-called Natural Science Method to the examination of psychical processes:

(1) The constant flux and caprice of mental events do not admit of the establishment of stable experimental conditions.

(2) Psychical processes offer no means for measurement or enumeration.

In the case of the special field of memory (learning, retention, reproduction) the second difficulty may be overcome to a certain extent. Among the external conditions of these processes some are directly accessible to measurement (the time, the number of repetitions). They may be employed in getting numerical values indirectly where that would not have been possible directly. We must not wait until the series of ideas committed to memory return to consciousness of themselves, but we must meet them halfway and renew them to such an extent that they may just be reproduced without error. The work requisite for this under certain conditions I take experimentally as a measure of the influence of these conditions; the differences in the work which appear with a change of conditions I interpret as a measure of the influence of that change.

Whether the first difficulty, the establishment of stable experimental conditions, may also be overcome satisfactorily cannot be decided *a priori*. Experiments must be made under conditions as far as possible the same, to see whether the results, which will probably deviate from one another when taken separately, will furnish constant mean values when collected to form larger groups. However, taken by itself, this is not sufficient to enable us to utilise such numerical results for the establishment of numerical relations of dependence in the natural science sense. Statistics is concerned with a great mass of constant mean values that do not at all arise from the frequent repetition of an ideally frequent occurrence and therefore cannot favor further insight into it. Such is the great complexity of our mental life that it is not possible to deny that constant mean values, when obtained, are of the nature of such statistical constants. To test that, I examine the distribution of the separate numbers represented in an average value. If it correspon

to the distribution found everywhere in natural science, where repeated observation of the same occurrence furnishes different separate values, I suppose—tentatively again—that the repeatedly examined psychical process in question occurred each time under conditions sufficiently similar for our purposes. This supposition is not compulsory, but is very probable. If it is wrong, the continuation of experimentation will presumably teach this by itself: the questions put from different points of view will lead to contradictory results.

Section 10. The Probable Error

The quantity which measures the compactness of the observed values obtained in any given case and which makes the formula which represents their distribution a definite one may, as has already been stated, be chosen differently. I use the so-called "probable error" (P.E.)—*i.e.*, that deviation above and below the mean value which is just as often exceeded by the separate values as not reached by them, and which, therefore, between its positive and negative limits, includes just half of all the observational results symmetrically arranged around the mean value. As is evident from the definition these values can be obtained from the results by simple enumeration; it is done more accurately by a theoretically based calculation.

If now this calculation is tried out tentatively for any group of observations, a grouping of these values according to the "law of errors" is recognised by the fact that between the sub-multiples and the multiples of the empirically calculated probable error there are obtained as many separate measures symmetrically arranged about a central value as the theory requires.

According to this out of 1,000 observations there should be:

Within the limits	Number of separate measures
$\pm \frac{1}{10}$ P.E.	54
$\pm \frac{1}{6}$ P.E.	89.5
$\pm \frac{1}{4}$ P.E.	134
$\pm \frac{1}{2}$ P.E.	264
$\pm$ P.E.	500
$\pm 1\frac{1}{2}$ P.E.	688
± 2 P.E.	823
$\pm 2\frac{1}{2}$ P.E.	908
± 3 P.E.	957
± 4 P.E.	993

If this conformity exists in a sufficient degree, then the mere statement of the probable error suffices to characterise the arrangement of all the observed values, and at the same time its quantity gives a serviceable measure for the compactness of the distribution around the central value—*i.e.*, for its exactness and trustworthiness.

As we have spoken of the probable error of the separate observations, ($P.E._o$), so can we also speak of the probable error of the measures of the central tendency, or mean values, ($P.E._m$). This describes in similar fashion the grouping which would arise for the separate mean values if the observation of the same phenomenon were repeated very many times and each time an equally great number of observations were combined into a central value. It furnishes a brief but sufficient characterisation of the fluctuations of the mean values resulting from repeated observations, and along with it a measure of the security and the trustworthiness of the results already found.

The $P.E._m$ is accordingly in general included in what follows. How it is found by calculation, again, cannot be explained here; suffice it that what it means be clear. It tells us, then, that, on the basis of the character of the total observations from which a mean value has just been obtained, it may be expected with a probability of 1 to 1 that the latter value departs from the presumably correct average by not more at the most than the amount of its probable error. By the presumably correct average we mean that one which would have been obtained if the observations had been indefinitely repeated. A larger deviation than this becomes improbable in the mathematical sense—*i.e.* there is a greater probability against it than for it. And, as a glance at the accompanying table shows us, the improbability of larger deviations increases with extreme rapidity as their size increases. The probability that the obtained average should deviate from the true one by more than 2½ times the probable error is only 92 to 908, therefore about 1/10; the probability for its exceeding four times the probable error is very slight 7 to 993 (1 to 142).

CHAPTER III

THE METHOD OF INVESTIGATION

Section 11. Series of Nonsense Syllables

In order to test practically, although only for a limited field, a way of penetrating more deeply into memory processes—and it is to these that the preceding considerations have been directed—I have hit upon the following method.

Out of the simple consonants of the alphabet and our eleven vowels and diphthongs all possible syllables of a certain sort were constructed, a vowel sound being placed between two consonants.[1]

These syllables, about 2,300 in number, were mixed together and then drawn out by chance and used to construct series of different lengths, several of which each time formed the material for a test.[2]

At the beginning a few rules were observed to prevent, in the construction of the syllables, too immediate repetition of similar sounds, but these were not strictly adhered to. Later they were abandoned and the matter left to chance. The syllables used each time were carefully laid aside till the whole number had been used, then they were mixed together and used again.

The aim of the tests carried on with these syllable series was, by means of repeated audible perusal of the separate series, to so impress them that immediately afterwards they could voluntarily just be reproduced. This aim was considered attained

[1] The vowel sounds employed were a, e, i, o, u, ä, ö, ü, au, ei, eu. For the beginning of the syllables the following consonants were employed: b, d, f, g, h, j, k, l, m, n, p, r, s, (= sz), t, w and in addition ch, sch, soft s, and the French j (19 altogether); for the end of the syllables f, k, l, m, n, p, r, s, (= sz) t, ch, sch (11 altogether). For the final sound fewer consonants were employed than for the initial sound, because a German tongue even after several years practise in foreign languages does not quite accustom itself to the correct pronunciation of the mediae at the end. For the same reason I refrained from the use of other foreign sounds although I tried at first to use them for the sake of enriching the material.

[2] I shall retain in what follows the designations employed above and call a group of several syllable series or a single series a "test." A number of "tests" I shall speak of as a "test series" or a "group of tests."

when, the initial syllable being given, a series could be recited at the first attempt, without hesitation, at a certain rate, and with the consciousness of being correct.

Section 12. Advantages of the Material

The nonsense material, just described, offers many advantages, in part because of this very lack of meaning. First of all, it is relatively simple and relatively homogeneous. In the case of the material nearest at hand, namely poetry or prose, the content is now narrative in style, now descriptive, or now reflective; it contains now a phrase that is pathetic, now one that is humorous; its metaphors are sometimes beautiful, sometimes harsh; its rhythm is sometimes smooth and sometimes rough. There is thus brought into play a multiplicity of influences which change without regularity and are therefore disturbing. Such are associations which dart here and there, different degrees of interest, lines of verse recalled because of their striking quality or their beauty, and the like. All this is avoided with our syllables. Among many thousand combinations there occur scarcely a few dozen that have a meaning and among these there are again only a few whose meaning was realised while they were being memorised.

However, the simplicity and homogeneity of the material must not be overestimated. It is still far from ideal. The learning of the syllables calls into play the three sensory fields, sight, hearing and the muscle sense of the organs of speech. And although the part that each of these senses plays is well limited and always similar in kind, a certain complication of the results must still be anticipated because of their combined action. Again, to particularise, the homogeneity of the series of syllables falls considerably short of what might be expected of it. These series exhibit very important and almost incomprehensible variations as to the ease or difficulty with which they are learned. It even appears from this point of view as if the differences between sense and nonsense material were not nearly so great as one would be inclined a priori to imagine. At least I found in the case of learning by heart a few cantos from Byron's "Don Juan" no greater range of distribution of the separate numerical measures than in the case of a series of nonsense syllables is

the learning of which an approximately equal time had been spent. In the former case the innumerable disturbing influences mentioned above seem to have compensated each other in producing a certain intermediate effect; whereas in the latter case the predisposition, due to the influence of the mother tongue, for certain combinations of letters and syllables must be a very heterogeneous one.

More indubitable are the advantages of our material in two other respects. In the first place it permits an inexhaustible amount of new combinations of quite homogeneous character, while different poems, different prose pieces always have something incomparable. It also makes possible a quantitative variation which is adequate and certain; whereas to break off before the end or to begin in the middle of the verse or the sentence leads to new complications because of various and unavoidable disturbances of the meaning.

Series of numbers, which I also tried, appeared impracticable for the more thorough tests. Their fundamental elements were too small in number and therefore too easily exhausted.

Section 13. Establishment of the Most Constant Experimental Conditions Possible

The following rules were made for the process of memorising.

1. The separate series were always read through completely from beginning to end; they were not learned in separate parts which were then joined together; neither were especially difficult parts detached and repeated more frequently. There was a perfectly free interchange between the reading and the occasionally necessary tests of the capacity to reproduce by heart. For the latter there was an important rule to the effect that upon hesitation the rest of the series was to be read through to the end before beginning it again.

2. The reading and the recitation of the series took place at a constant rate, that of 150 strokes per minute. A clockwork metronome placed at some distance was at first used to regulate the rate; but very soon the ticking of a watch was substituted, that being much simpler and less disturbing to the attention. The mechanism of escapement of most watches swings 300 times per minute.

3. Since it is practically impossible to speak continuously without variation of accent, the following method was adopted to avoid irregular variations: either three or four syllables were united into a measure, and thus either the 1st, 4th, 7th, or the 1st, 5th, 9th . . . syllables were pronounced with a slight accent. Stressing of the voice was otherwise, as far as possible, avoided.

4. After the learning of each separate series a pause of 15 seconds was made, and used for the tabulation of results. Then the following series of the same test was immediately taken up.

5. During the process of learning, the purpose of reaching the desired goal as soon as possible was kept in mind as much as was feasible. Thus, to the limited degree to which conscious resolve is of influence here, the attempt was made to keep the attention concentrated on the tiresome task and its purpose. It goes without saying that care was taken to keep away all outer disturbances in order to make possible the attainment of this aim. The smaller distractions caused by carrying on the test in various surroundings were also avoided as far as that could be done.

6. There was no attempt to connect the nonsense syllables by the invention of special associations of the mnemotechnik type; learning was carried on solely by the influence of the mere repetitions upon the natural memory. As I do not possess the least practical knowledge of the mnemotechnical devices, the fulfillment of this condition offered no difficulty to me.

7. Finally and chiefly, care was taken that the objective conditions of life during the period of the tests were so controlled as to eliminate too great changes or irregularities. Of course, since the tests extended over many months, this was possible only to a limited extent. But, even so, the attempt was made to conduct, under as similar conditions of life as possible, those tests the results of which were to be directly compared. In particular the activity immediately preceding the test was kept as constant in character as was possible. Since the mental as well as the physical condition of man is subject to an evident periodicity of 24 hours, it was taken for granted that like experimental conditions are obtainable only at like times of day. However, in order to carry out more than one test in a given day, different experiments were occasionally carried on together

at different times of day. When too great changes in the outer and inner life occurred, the tests were discontinued for a length of time. Their resumption was preceded by some days of renewed training varying according to the length of the interruption.

Section 14. Sources of Error

The guiding point of view in the selection of material and in determining the rules for its employment was, as is evident, the attempt to simplify as far as possible, and to keep as constant as possible, the conditions under which the activity to be observed, that of memory, came into play. Naturally the better one succeeds in this attempt the more does he withdraw from the complicated and changing conditions under which this activity takes place in ordinary life and under which it is of importance to us. But that is no objection to the method. The freely falling body and the frictionless machine, etc., with which physics deals, are also only abstractions when compared with the actual happenings in nature which are of import to us. We can almost nowhere get a direct knowledge of the complicated and the real, but must get at them in roundabout ways by successive combinations of experiences, each of which is obtained in artificial, experimental cases, rarely or never furnished in this form by nature.

Meanwhile the fact that the connection with the activity of memory in ordinary life is for the moment lost is of less importance than the reverse, namely, that this connection with the complications and fluctuations of life is necessarily still a too close one. The struggle to attain the most simple and uniform conditions possible at numerous points naturally encounters obstacles that are rooted in the nature of the case and which thwart the attempt. The unavoidable dissimilarity of the material and the equally unavoidable irregularity of the external conditions have already been touched upon. I pass next to two other unsurmountable sources of difficulty.

By means of the successive repetitions the series are, so to speak, raised to ever higher levels. The natural assumption would be that at the moment when they could for the first time be reproduced by heart the level thus attained would always be the same. If only this were the case, i.e., if this characteristic

first reproduction were everywhere an invariable objective sign of an equally invariable fixedness of the series, it would be of real value to us. This, however, is not actually the case. The inner conditions of the separate series at the moment of the first possible reproduction are not always the same, and the most that can be assumed is that in the case of these different series these conditions always oscillate about the same degree of inner surety. This is clearly seen if the learning and repeating of the series is continued after that first spontaneous reproduction of the series has been attained. As a general thing the capacity for voluntary reproduction persists after it has once been reached. In numerous cases, however, it disappears immediately after its first appearance, and is regained only after several further repetitions. This proves that the predisposition for memorising the series, irrespective of their differences of a larger sort according to the time of day, to the objective and subjective conditions, etc., is subject to small variations of short duration, whether they be called oscillations of attention or something else. If, at the very instant when the material to be memorised has almost reached the desired degree of surety, a chance moment of especial mental clearness occurs, then the series is caught on the wing as it were, often to the learner's surprise; but the series cannot long be retained. By the occurrence of a moment of special dullness, on the other hand, the first errorless reproduction is postponed for a while, although the learner feels that he really is master of the thing and wonders at the constantly recurring hesitations. In the former case, in spite of the homogeneity of the external conditions, the first errorless reproduction is reached at a point a little below the level of retention normally connected with it. In the latter case it is reached at a point a little above that level. As was said before, the most plausible conjecture to make in this connection is that these deviations will compensate each other in the case of large groups.

Of the other source of error, I can only say that it may occur and that, when it does, it is a source of great danger. I mean the secret influence of theories and opinions which are in the process of formation. An investigation usually starts out with definite presuppositions as to what the results will be. But if this is not the case at the start, such presuppositions form gradu-

ally in case the experimenter is obliged to work alone. For it is impossible to carry on the investigations for any length of time without taking notice of the results. The experimenter must know whether the problem has been properly formulated or whether it needs completion or correction. The fluctuations of the results must be controlled in order that the separate observations may be continued long enough to give to the mean value the certainty necessary for the purpose in hand. Consequently it is unavoidable that, after the observation of the numerical results, suppositions should arise as to general principles which are concealed in them and which occasionally give hints as to their presence. As the investigations are carried further, these suppositions, as well as those present at the beginning, constitute a complicating factor which probably has a definite influence upon the subsequent results. It goes without saying that what I have in mind is not any consciously recognised influence but something similar to that which takes place when one tries to be very unprejudiced or to rid one's self of a thought and by that very attempt fosters that thought or prejudice. The results are met half way with an anticipatory knowledge, with a kind of expectation. Simply for the experimenter to say to himself that such anticipations must not be allowed to alter the impartial character of the investigation will not by itself bring about that result. On the contrary, they do remain and play a rôle in determining the whole inner attitude. According as the subject notices that these anticipations are confirmed or not confirmed (and in general he notices this during the learning), he will feel, if only in a slight degree, a sort of pleasure or surprise. And would you not expect that, in spite of the greatest conscientiousness, the surprise felt by the subject over especially startling deviations, whether positive or negative, would result, without any volition on his part, in a slight change of attitude? Would he not be likely to exert himself a little more here and to relax a little more there than would have been the case had he had no knowledge or presupposition concerning the probable numerical value of the results? I cannot assert that this is always or even frequently the case, since we are not here concerned with things that can be directly observed, and since numerous results in which such secret warping of the truth might be expected show evident independence

of it. All I can say is, we must expect something of the sort from our general knowledge of human nature, and in any investigations in which the inner attitude is of very great importance, as for example in experiments on sense perception, we must give special heed to its misleading influence.

It is evident how this influence in general makes itself felt. With average values it would tend to level the extremes; where especially large or small numbers are expected it would tend to further increase or decrease the values. This influence can only be avoided with certainty when the tests are made by two persons working together, one of whom acts as subject for a certain time without raising any questions concerning the purpose or the result of the investigations. Otherwise help can be obtained only by roundabout methods, and then, probably, only to a limited extent. The subject, as I myself always did, can conceal from himself as long as possible the exact results. The investigation can be extended in such a way that the upper limits of the variables in question are attained. In this way, whatever warping of the truth takes place becomes relatively more difficult and unimportant. Finally, the subject can propose many problems which will appear to be independent of each other in the hope that, as a result, the true relation of the interconnected mental processes will break its way through.

To what extent the sources of error mentioned have affected the results given below naturally cannot be exactly determined. The absolute value of the numbers will doubtless be frequently influenced by them, but as the purpose of the tests could never have been the precise determination of absolute values, but rather the attainment of comparative results (especially in the numerical sense) and relatively still more general results, there is no reason for too great anxiety. In one important case (§ 38) I could directly convince myself that the exclusion of all knowledge concerning the character of the results brought about no change; in another case where I myself could not eliminate a doubt I called especial attention to it. In any case he who is inclined *a priori* to estimate very highly the unconscious influence of secret wishes on the total mental attitude will also have to take into consideration that the secret wish to find objective truth and not with disproportionate toil to place the creation of his own fancy upon feet of clay—that this wish,

I say, may also claim a place in the complicated mechanism of these possible influences.

Section 15. Measurement of Work Required

The number of repetitions which were necessary for memorising a series up to the first possible reproduction was not originally determined by counting, but indirectly by measuring in seconds the time that was required to memorise it. My purpose was in this way to avoid the distraction necessarily connected with counting; and I could assume that there was a proportional relation existing between the times and the number of repetitions occurring at any time in a definite rhythm. We could scarcely expect this proportionality to be perfect, since, when only the time is measured, the moments of hesitation and reflection are included, which is not true when the repetitions are counted. Difficult series in which hesitation will occur relatively more frequently, will, by the method of time measurement, get comparatively greater numbers, the easier series will get comparatively smaller numbers than when the repetitions are counted. But with larger groups of series a tolerably equal distribution of difficult and equal series may be taken for granted. Consequently the deviations from proportionality will compensate themselves in a similar manner in the case of each group.

When, for certain tests, the direct counting of the repetitions became necessary, I proceeded in the following manner. Little wooden buttons measuring about 14 mms. in diameter and 4 mms. at their greatest thickness were strung on a cord which would permit of easy displacement and yet heavy enough to prevent accidental slipping. Each tenth piece was black; the others had their natural color. During the memorisation the cord was held in the hand and at each new repetition a piece was displaced some centimeters from left to right. When the series could be recited, a glance at the cord, since it was divided into tens, was enough to ascertain the number of repetitions that had been necessary. The manipulation required so little attention that in the mean values of the time used (which was always tabulated at the same time) no lengthening could be noted as compared with earlier tests.

By means of this simultaneous measurement of time and repe-

titions incidental opportunity was afforded for verifying and more accurately defining that which had been foreseen and which has just been explained with regard to their interrelation. When the prescribed rhythm of 150 strokes per minute was precisely maintained, each syllable would take 0.4 second; and when the simple reading of the series was interrupted by attempts to recite it by heart, the unavoidable hesitations would lengthen the time by small but fairly uniform amounts. This, however, did not hold true with any exactness; on the contrary, the following modifications appeared.

When the direct reading of the series predominated, a certain forcing, an acceleration of the rhythm, occurred which, without coming to consciousness, on the whole lowered the time for each syllable below the standard of 0.4 sec.

When there was interchange between reading and reciting, however, the lengthening of the time was not in general constant, but was greater with the longer series. In this case, since the difficulty increases very rapidly with increasing length of the series, there occurs a slowing of the tempo, again involuntary and not directly noticeable. Both are illustrated by the following table.

Series of 16 syllables, for the most part read	Each syllable required the average time of	Number of series	Number of syllables
8 times	0.398 sec.	60	960
16 "	0.399 "	108	1728

Series of X syllables	Were in part read, in part recited on an average Y times	Each syllable required an average time of Z secs.	Number of series	Number of syllables
X=	Y=	Z=		
12	18	0.416	63	756
16	31	0.427	252	4032
24	45	0.438	21	504
36	56	0.459	14	504

As soon as this direction of deviation from exact proportionality was noticed there appeared in the learning a certain conscious reaction against it.

Finally, it appeared that the probable error of the time measurements was somewhat larger than that of the repetitions. This relation is quite intelligible in the light of the explanations given above. In the case of the time measurements the larger values, which naturally occurred with the more difficult series, were relatively somewhat greater than in the case of the number of repetitions, because relatively they were for the most part lengthened by the hesitations; conversely, the smaller times were necessarily somewhat smaller relatively than the number of repetitions, because in general they corresponded to the easier series. The distribution of the values in the case of the times is therefore greater than that of the values in the case of the repetitions.

The differences between the two methods of reckoning are, as is readily seen, sufficiently large to lead to different results in the case of investigations seeking a high degree of exactness. That is not the case with the results as yet obtained; it is therefore immaterial whether the number of seconds is used or that of the repetitions.

Decision cannot be given *a priori* as to which method of measurement is more correct—*i.e.*, is the more adequate measure of the mental work expended. It can be said that the impressions are due entirely to the repetitions, they are the thing that counts; it can be said that a hesitating repetition is just as good as a simple fluent reproduction of the line, and that both are to be counted equally. But on the other hand it may be doubted that the moments of recollection are merely a loss. In any case a certain display of energy takes place in them: on the one hand, a very rapid additional recollection of the immediately preceding words occurs, a new start, so to speak, to get over the period of hesitation; on the other hand, there is heightened attention to the passages following. If with this, as is probable, a firmer memorisation of the series takes place, then these moments have a claim upon consideration which can only be given to them through the measurement of the times.

Only when a considerable difference in the results of the two kinds of tabulation appears will it be possible to give one the preference over the other. That one will then be chosen which gives the simpler formulation of the results in question.

Section 16. Periods of the Tests

The tests were made in two periods, in the years 1879-80 and 1883-84, and extended each over more than a year. During a long time preliminary experiments of a similar nature had preceded the definite tests of the first period, so that, for all results communicated, the time of increasing skill may be considered as past. At the beginning of the second period I was careful to give myself renewed training. This temporal distribution of the tests with a separating interval of more than three years gives the desired possibility of a certain mutual control of most of the results. Frankly, the tests of the two periods are not strictly comparable. In the case of the tests of the first period, in order to limit the significance of the first fleeting grasp[1] of the series in moments of special concentration, it was decided to study the series until two successive faultless reproductions were possible. Later I abandoned this method, which only incompletely accomplished its purpose, and kept to the first fluent reproduction. The earlier method evidently in many cases resulted in a somewhat longer period of learning. In addition there was a difference in the hours of the day appointed for the tests. Those of the later period all occurred in the afternoon hours between one and three o'clock; those of the earlier period were unequally divided between the hours of 10-11 A. M., 11-12 A. M., and 6-8 P. M., which for the sake of brevity I shall designate A, B, and C.

[1] Described in § 14.

CHAPTER IV

THE UTILITY OF THE AVERAGES OBTAINED

Section 17. Grouping of the Results of the Tests

The first question which awaits an answer from the investigations carried out in the manner described is, as explained in sections 7 and 8, that of the nature of the averages obtained. Are the lengths of time required for memorising series of a certain length, under conditions as nearly identical as possible, grouped in such a way that we may be justified in considering their average values as measures in the sense of physical science, or are they not?

If the tests are made in the way described above, namely, so that several series are always memorised in immediate succession, such a type of grouping of the time records could scarcely be expected. For, as the time devoted to learning at a given sitting becomes extended, certain variable conditions in the separate series come into play, the fluctuations of which we could not very well expect, from what we know of their nature, to be distributed symmetrically around a mean value. Accordingly the grouping of the results must be an asymmetrical one and cannot correspond to the "law of error." Such conditions are the fluctuations of attention and the decreasing mental freshness, which, at first very quickly and then more and more slowly, gives way to a certain mental fatigue. There are no limits, so to speak, to the slowing down of the learning processes caused by unusual distractions; as a result of these the time for learning a series may occasionally be increased to double that of its average value or more. The opposite effect, that of an unusual exertion, cannot in the very nature of the case overstep a certain limit. It can never reduce the learning time to zero.

If, however, groups of series equal in number and learned in immediate succession are taken, these disturbing influences

may be considered to have disappeared or practically so. The decrease in mental vigour in one group will be practically the same as that in another. The positive and negative fluctuations of attention which under like conditions occur during a quarter or half hour are approximately the same from day to day. All that is necessary to ask, then, is: Do the times necessary for learning equal groups of series exhibit the desired distribution?

I can answer this question in the affirmative with sufficient certainty. The two longest series, obtained under conditions similar to each other, which I possess, are, to be sure, not large in the above-mentioned theoretical sense; they suffer, moreover, from the disadvantage that they originated at times separated by comparatively long intervals during which there were necessarily many changes in the conditions. In spite of this, their grouping comes as near as could be expected to the one demanded by the theory.

The first test series taken during the years 1879-80 comprises 92 tests. Each test consisted in memorising eight series of 13 syllables each, which process of learning was continued until two reproductions of each series were possible. The time required for all eight series taken together including the time for the two reproductions (but of course not for the pauses, see p. 25, 4) amounted to an average of 1,112 seconds with a probable error of observation of $\pm$ 76. The fluctuations of the results were, therefore, very significant: only half of the numbers obtained fell between the limits 1,036 and 1,188, the other half was distributed above and below these limits. In detail the grouping of the numbers is as follows:

Within the limits	*i.e.* within the deviation	Number of deviations	
		By actual count	Calculated from theory
1/10 P.E.	$\pm$ 7	6	5
1/6 P.E.	$\pm$ 12	10	8.2
1/4 P.E.	$\pm$ 19	13	12.3
1/2 P.E.	$\pm$ 38	30	24.3
P.E.	$\pm$ 76	45	46.0
1½ P.E.	$\pm$ 114	61	63.4
2 P.E.	$\pm$ 152	73	75.6
2½ P.E.	$\pm$ 190	84	83.6
3 P.E.	$\pm$ 228	88	88.0

In the interval, 1/4 P.E. to 1/2 P.E., there occurs a slight piling up of values which is compensated for by a greater lack in the succeeding interval, 1/2 P.E. to P.E. Apart from this, the correspondence between the calculated and the actual results is satisfactory. The symmetry of the distribution leaves something to be desired. The values below the average preponderate a little in number, those above preponderate a little in amount of deviation: only two of the largest eight deviations are below the mean value. The influence of attention referred to above, the fluctuations of which in the separate series show greater deviations toward the upper limit than toward the lower, has not, therefore, been quite compensated by the combination of several series.

The correctness of the observations and the correspondence of their distributions with the one theoretically demanded are greatly improved in the second large series of tests. The latter comprises the results of 84 series of tests taken during the years 1883-84. Each test consisted in memorising six series of 16 syllables each, carried on in each case to the first errorless reproduction. The whole time necessary for this amounted to 1,261 seconds with the probable error of observation of ± 48.4—*i.e.*, half of all the 84 numbers fell within the limits 1,213-1,309. The exactness of the observations thus had greatly increased as compared with the former series of tests:[1]

The interval included by the probable error amounts to only 7½ per cent of the mean value as against 14 per cent in the earlier tests. In detail the numbers are distributed as follows:

[1] Of course, the exactness obtained here cannot stand comparison with physical measurements, but it can very well be compared with physiological ones, which would naturally be the first to be thought of in this connection. To the most exact of physiological measurements belong the last determinations of the speed of nervous transmission made by Helmholtz and Baxt. One record of these researches published as an illustration of their accuracy (Mon. Ber. d. Berl. Akad. 1870, S. 191) after proper calculation gives a mean value of 4.268 with the probable error of observation, 0.101. The interval it includes amounts, therefore, to 5 per cent of the mean value. All former determinations are much more inaccurate. In the case of the most accurate test-series of the first measurements made by Helmholtz, that interval amounts to about 50 per cent of the mean value (*Arch. f. Anat. u.. Physiol.* 1850, S. 340). Even Physics, in the case of its pioneer investigations, has often been obliged to put up with a less degree of accuracy in its numerical results. In the case of his first determinations of the mechanical equivalent of heat Joule found the number 838, with a probable error of observation of 97. (*Phil. Mag.*, 1843, p. 435 ff.)

Within the limits	*i.e.*, within the deviation	Number of deviations	
		By actual count	Calculated from theory
1/12 P.E.	± 4	4	4.5
1/6 P.E.	± 8	7	7.6
1/4 P.E.	± 12	12	11.3
1/2 P.E.	± 24	23	22.2
P.E.	± 48	44	42.0
1½ P.E.	± 72	57	57.8
2 P.E.	± 96	68	69.0
2½ P.E.	± 121	75	76.0
3 P.E.	± 145	81	80.0

The symmetry of distribution is here satisfactorily maintained apart from the numbers, which are unimportant on account of their smallness.

Within the limits	Deviations	
	Above	Below
1/6 P.E.	5	2
1/4 P.E.	7	5
1/2 P.E.	13	10
P.E.	20	24
1½ P.E.	28	29
2 P.E.	34	34
2½ P.E.	37	38
3 P.E.	40	41

The deviation which is greatest absolutely is toward the lower limit.

If several of our series of syllables were combined into groups and then memorised separately, the length of time necessary to memorise a whole group varied greatly, to be sure, when repeated tests were taken; but, in spite of this, when taken as a whole they varied in a manner similar to that of the measures of the ideally homogeneous processes of natural science, which also vary from each other. So, at least in experimental fashion, it is allowable to use the mean values obtained from the numerical results for the various tests in order to establish the existence of causal relations just as natural science does that by means of its constants.

The number of series of syllables which is to be combined into

a single group, or test, is naturally indeterminate. It must be expected, however, that as the number increases, the correspondence between the distribution of the times actually found and those calculated in accordance with the law of errors will be greater. In practice the attempt will be made to increase the number to such a point that further increase and the closer correspondence resulting will no longer compensate for the time required. If the number of the series in a given test is lessened, the desired correspondence will also presumably decrease. However, it is desirable that even then the approximation to the theoretically demanded distribution remain perceptible.

Even this requirement is fulfilled by the numerical values obtained. In the two largest series of tests just described, I have examined the varying length of time necessary for the memorisation of the first half of each test. In the older series, these are the periods required by each 4 series of syllables, in the more recent series the periods required by each 3 of them taken together. The results are as follows:

1. In the former series: mean value (m) $= 533$ (P.E.$_0$) $= \pm 51$.

DISTRIBUTION OF THE SEPARATE VALUES

Within the limits	*i.e.*, within the deviation	Number of deviations		Of these deviations there occur	
		By actual count	Calculated from theory	Below	Above
1/10 P.E.	± 5	2	5.0	2	0
1/6 P.E.	± 8	4	8.2	3	1
1/4 P.E.	± 12	6	12.3	4	2
1/2 P.E.	± 25	21	24.3	9	12
P.E.	± 51	48	46.0	24	24
1½ P.E.	± 76	61	63.4	30	31
2 P.E.	± 102	76	75.6	37	39
2½ P.E.	± 127	85	83.6	42	43
3 P.E.	± 153	89	88.0	45	44

2. In the later series: $m = 620$, $P.E._o = \pm 44$.

DISTRIBUTION OF THE SEPARATE VALUES

Within the limits	*i.e.*, within the deviation	Number of deviations		Of these deviations there occur	
		By actual count	Calculated from theory	Below	Above
1/10 P.E.	± 4	3	4.5	1	2
1/8 P.E.	± 7	5	7.6	3	2
1/4 P.E.	± 11	11	11.3	6	5
1/2 P.E.	± 22	25	22.2	13	12
P.E.	± 44	44	42.0	21	23
1½ P.E.	± 66	56	57.8	29	27
2 P.E.	± 88	71	69.0	38	33
2½ P.E.	± 110	76	76.0	41	35
3 P.E.	± 132	79	80.0	42	37

By both tables the supposition mentioned above of the existence of a less perfect but still perceptible correspondence between the observed and calculated distribution of the numbers is well confirmed.

Exactly the same approximate correspondence must be presupposed if, instead of decreasing the number of series combined into a test, the total number of tests is made smaller. In this case also I will add some confirmatory summaries.

I possess two long test series, made at the time of the earlier tests, which were obtained under the same conditions as the above mentioned series but at the later times of the day, B. and C. One of these, B, comprised 39 tests of 6 series each, the other, C, 38 tests of 8 series each, each series containing 13 syllables. The results obtained were as follows:

1. For the tests at time B: $m = 871$, $P.E._0 = \pm 63$.

DISTRIBUTION OF THE SEPARATE VALUES

Within the limits	Number of deviations	
	Counted	Calculated
¼ P.E.	4	5
½ P.E.	10	10.3
P.E.	21	19.5
1½ P.E.	28	26.8
2 P.E.	32	32.0
2½ P.E.	35	35.4
3 P.E.	37	37.3

2. For the tests of time C: $m = 1{,}258$, $P.E._0 = \pm 60$.

DISTRIBUTION OF THE SEPARATE VALUES

Within the limits	Number of deviations	
	Counted	Calculated
¼ P.E.	7	5.0
½ P.E.	10	10.0
P.E.	19	19.0
1½ P.E.	26	26.0
2 P.E.	31	31.0
2½ P.E.	34	34.5
3 P.E.	36	36.4

In addition I mention a series of only twenty tests, with which I shall conclude this summary. Each test consisted of the learning of eight separate series of thirteen syllables each, which had been memorised once one month before. The average was in this case 892 seconds with a probable error of observation of 54. The single values were grouped as follows:

Within the limits	Number of deviations	
	Counted	Calculated
¼ P.E.	3	2.7
½ P.E.	5	5.3
P.E.	10	10.0
1½ P.E.	12	13.8
2 P.E.	17	16.5
2½ P.E.	19	18.2
3 P.E.	20	19.1

Although the number of the tests was so small, the accordance between the calculation by theory and the actual count of deviations is in all these cases so close that the usefulness of the mean values will be admitted, the wide limits of error being, of course, taken into consideration.

Section 18. Grouping of the Results of the Separate Series

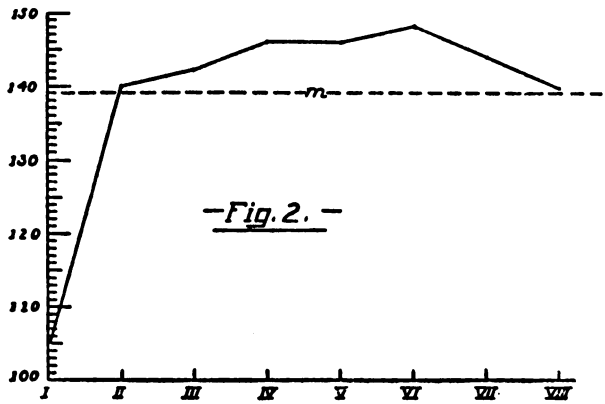
The previously mentioned hypotheses concerning the grouping of the times necessary for learning the separate series were naturally not merely theoretical suppositions, but had already been confirmed by the groupings actually found. The two large series of tests mentioned above, one consisting of 92 tests of eight single series each, and the other of 84 tests of 6 single series each, thus giving 736 and 504 separate values respectively, afford a sufficiently broad basis for judgment. Both groups of numbers, and both in the same way, show the following peculiarities:

1. The distribution of the arithmetical values above their mean is considerably looser and extends farther than below the mean. The most extreme values above lie 2 times and 1.8 times, respectively, as far from the mean as the most distant of those below.

2. As a result of this dominance by the higher numbers the mean is displaced upward from the region of the densest distribution, and as a result the deviations below get the preponderance in number. There occur respectively 404 and 266 deviations below as against 329 and 230 above.

3. The number of deviations from the region of densest distribution towards both limits does not decrease uniformly—as one would be very much inclined to expect from the relatively large numbers combined—but several maxima and minima of density are distinctly noticeable. Therefore constant sources of error were at work in the production of the separate values—*i.e.*, in the memorisation of the separate series. These resulted on the one hand in an unsymmetrical distribution of the numbers, and on the other hand in an accumulation of them in certain regions. In accordance with the investigations already presented in this chapter, it can only be supposed that these influences compensated each other when the values of several series learned in succession were combined.

I have already mentioned as the probable cause of this unsymmetrical distribution the peculiar variations in the effect of high degrees of concentration of attention and distraction. It would naturally be supposed that the position of the separate series within each test is the cause of the repeated piling up of values on each side of the average. If, in the case of a large test-series, the values are summed up for the first, the second, and third series, etc., and the average of each is taken, these average values vary greatly, as might be expected. The separate values are grouped about their mean with only tolerable approximation to the law of error, but yet they are, on the whole, distributed most densely in its region, and these separate regions of dense distribution must of course appear in the total result.



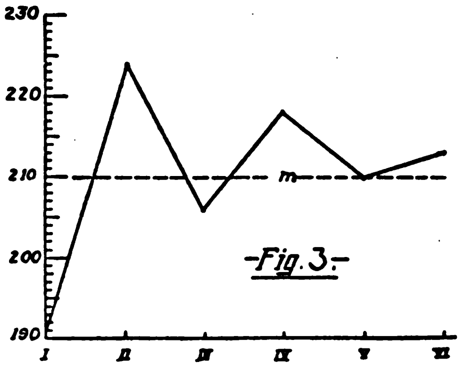
—Fig. 2. —

The following may be added by way of supplement: on account of the mental fatigue which increases gradually during the course of a test-series the mean values ought to increase with the number of the series; but this does not prove to be the case.

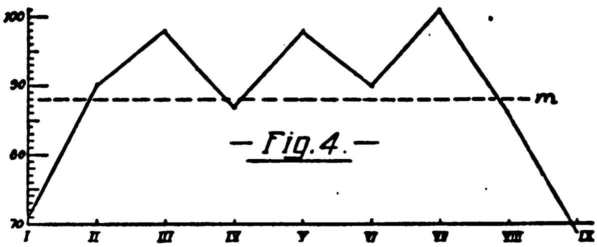
Only in one case have I been able to notice anything corresponding to this hypothesis, namely, in the large and therefore important series of 92 tests consisting of eight series of 13 syllables each. In this case the mean values for the learning of the 92 first series, the 92 second series, etc., were found to be 105, 140, 142, 146, 146, 148, 144, 140 seconds, the relative lengths of which Fig. 2 exhibits. For all the rest of the cases which I investigated the typical fact is, on the contrary, rather such a course of the numbers as was true in the case of the

series of 84 tests of six series of 16 syllables each and as is shown in Fig. 3.

The mean values here were 191, 224, 206, 218, 210, 213 seconds. They start in, as may be seen, considerably below the average, but rise immediately to a height which is not again reached in the further course of the test, and they then oscillate



rather decidedly. An analogous course is shown by the numbers in the 7 tests of nine 12-syllable series, namely: 71, 90, 98, 87, 98, 90, 101, 86, 69 (Fig. 4).



Furthermore the values for 39 tests of six series of 13 syllables each obtained in time B were as follows: 118, 150, 158, 147, 155, 144 (Fig. 5 lower curve).

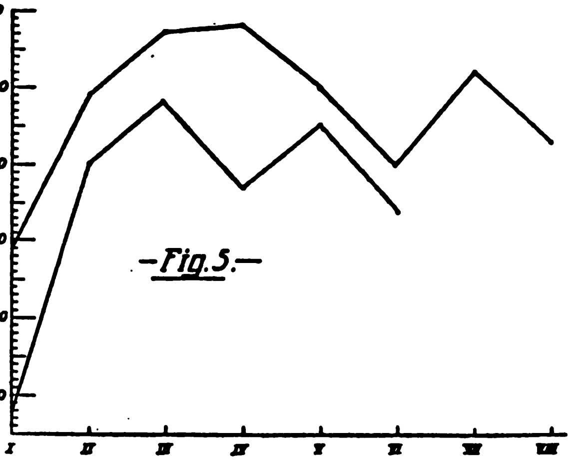
Those for 38 tests with eight 13 syllable series of time C were 139, 159, 167, 168, 160, 150, 162, 153 (Fig. 5 upper curve).

Finally the numbers obtained from seven tests with six stanzas of Byron's "Don Juan" were: 189, 219, 171, 204, 183, 229.

Even in the case of the first mentioned contradictory group of tests a grouping of the separate mean values harmonising with the normal one occurs if, instead of all the 92 tests being taken into consideration at once, they are divided into several parts—*i.e.*, if tests are combined which were taken at about the same time and under about the same conditions.

The conclusion cannot be drawn from these numerical results that the mental fatigue which gradually increased during the twenty minute duration of the tests did not exert any influence.

It can only be said that the supposed influence of the latter upon the numbers is far outweighed by another tendency which



-Fig.5.-

would not *a priori* be so readily suspected, namely the tendency of comparatively low values to be followed by comparatively high ones and vice versa. There seems to exist a kind of periodical oscillation of mental receptivity or attention in connection with which the increasing fatigue expresses itself by fluctuations around a median position which is gradually displaced.[1]

[1] If it should ever become a matter of interest, the attempt might be made to define numerically the different effects of that tendency in different cases. For the probable errors of observation for the numerical values of series-groups afford a measure for the influence of accidental disturbances to which the memorisation is daily exposed. If now the learning of the separate series in general were exposed to the same or similar variations of condition as occur from test to test, then according to the fundamental principles of the theory of errors, a probable error of observation calculated directly from the separate values would relate itself to the one just mentioned as 1 to $\sqrt{n}$, where "n" denotes the number of separate series combined into a test. If, however, as is the case here, special influences assert themselves during the memorisation of these

After orienting ourselves thus concerning the nature and value of the numerical results gained from the complete memorisations, we shall now turn to the real purpose of the investigation, namely the numerical description of causal relations.

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separate series, and if such influences tend to separate the values further than other variations of conditions would do, the "$P.E._o$" calculated from the separate values must turn out somewhat too great, and the just mentioned proportion consequently too small, and the stronger the influences are, the more must this be the case.

An examination of the actual relations is, to be sure, a little difficult, but fully confirms the statements. In the 84 tests, consisting of six series of 16 syllables each, the $\sqrt{n} = 2.45$. We found 48.4 to be the probable error of observation of the 84 tests. The probable error of the 504 separate values is 31.6. The quotient 31.6 : 48.4 is 1.53; therefore not quite ⅔ of the value of $\sqrt{n}$.

CHAPTER V

RAPIDITY OF LEARNING SERIES OF SYLLABLES AS A FUNCTION OF THEIR LENGTH

Section 19. Tests Belonging to the Later Period

It is sufficiently well known that the memorisation of a series of ideas that is to be reproduced at a later time is more difficult, the longer the series is. That is, the memorisation not only requires more time taken by itself, because each repetition lasts longer, but it also requires more time relatively because an increased number of repetitions becomes necessary. Six verses of a poem require for learning not only three times as much time as two but considerably more than that.

I did not investigate especially this relation of dependence, which of course becomes evident also in the first possible reproduction of series of nonsense syllables, but incidentally I obtained a few numerical values for it which are worth putting down, although they do not show particularly interesting relations.

The series in question comprised (in the case of the tests of the year 1883-84), 12, 16, 24, or 36 syllables each, and 9, 6, 3, or 2 series were each time combined into a test.

For the number of repetitions necessary in these cases to memorise the series up to the first errorless reproduction (and including it) the following numerical results were found:

X series	Y syllables each	Required together an average of Z repetitions	Probable error of average values	Number of tests
X=	Y=	Z=		
9	12	158	± 3.4	7
6	16	186	± 0.9	42
3	24	134	± 2.9	7
2	36	112	± 4.0	7

In order to make the number of repetitions comparable it is necessary, so to speak, to reduce them to a common denominator and to divide them each time by the number of the series. In this way it is found out how many repetitions relatively were necessary to learn by heart the single series, which differ from each other only in the number of syllables, and which each time had been taken together with as many others of the same kind as would make the duration of the whole test from fifteen to thirty minutes.[1]

However, a conclusion can be drawn from the figures from the standpoint of decrease in number of syllables. The question can be asked: What number of syllables can be correctly recited after only one reading? For me the number is usually seven. Indeed I have often succeeded in reproducing eight syllables, but this has happened only at the beginning of the tests and in a decided minority of the cases. In the case of six syllables on the other hand a mistake almost never occurs; with them, therefore, a single attentive reading involves an unnecessarily large expenditure of energy for an immediately following reproduction.

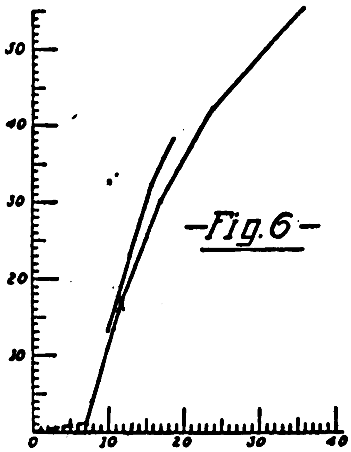
If this latter pair of values is added, the required division made, and the last faultless reproduction subtracted as not necessary for the learning, then the following table results.

Number of syllables in a series	Number of repetitions necessary for first errorless reproduction (exclusive of it)	Probable error
7	1	
12	16.6	± 1.1
16	30.0	± 0.4
24	44.0	± 1.7
36	55.0	± 2.8

[1] The objection might be made that, by means of this division, recourse is made directly to the averages for the memorising of the single series, and that in this way the result of the Fourth Chapter is disregarded. For, according to that, the averages of the numbers obtained from groups of series could indeed be used for investigation into relations of dependence, but the averages obtained from separate series could not be so used. I do not claim, however, that the above numbers, thus obtained by division, form the correct average for the numbers belonging to the separate series, *i.e.*, that the latter group themselves according to the law of errors. But the numbers are to be considered as averages for groups of series, and, for the sake of a better comparison with others—a condition which in the nature of the case could not be everywhere the same—is made the same by division. The probable error, the measure of their accuracy, has not been calculated from the numbers for the separate series but from those for the groups of series.

The longer of the two adjoining curves of Fig. 6 illustrates the regular course of these numbers with approximate accuracy for such a small number of tests. As Fig. 6 shows, in the cases examined, the number of repetitions necessary for the memorisation of series in which the number of syllables progressively increased, itself increases with extraordinary rapidity with the increase in number of the syllables.

At first the ascent of the curve is very steep, but later on it appears to gradually flatten out. For the mastery of five times



—Fig. 6—

the number of syllables that can be reproduced after but one reading—*i.e.*, after about 3 seconds—over 50 repetitions were necessary, requiring an uninterrupted and concentrated effort for fifteen minutes.

The curve has its natural starting point in the zero point of the co-ordinates. The short initial stretch up to the point, $x = 7$, $y = 1$, can be explained thus: in order to recite by heart series of 6, 5, 4, etc., syllables one reading, of course, is all that is necessary. In my case this reading does not require as much attention as does the 7-syllable one, but can become more and more superficial as the number of syllables decreases.

Section 20. Tests Belonging to the Earlier Period

It goes without saying that since the results reported were obtained from only one person they have meaning only as related to him. The question arises whether they are for this individual of a general significance—*i.e.*, whether, by repetition of the tests at another time, they could be expected to show approximately the same amount and grouping.

A series of results from the earlier period furnishes the desired possibility of a control in this direction. They, again, have been obtained incidentally (consequently uninfluenced by expectations and suppositions) and from tests made under different conditions than those mentioned. These earlier tests occurred at an earlier hour of the day and the learning was continued until the separate series could be recited twice in succession without mistake. A test comprised

15 series of 10 syllables each,
or 8 " " 13 " "
or 6 " " 16 " "
or 4 " " 19 " "

So, again, four different lengths of series have been taken into account, but their separate values lie much closer together.

Since the repetitions—which are in question here—were not counted at all in the earlier period, their number had to be calculated from the times. For this purpose the table on p. 31 has been used after corresponding interpolation. If the numbers found are immediately reduced to one series each, and if along with it the two repetitions representing the recitation are subtracted as above, we obtain:

Number of syllables in a series	Number of repetitions necessary for two errorless reproductions (exclusive of them)	Probable error[1]	Number of tests
10	13	± 1.	16
13	23	± 0.5	92
16	32	± 1.2	6
19	38	± 2.0	11

[1] The probable errors are based upon calculation and have only an approximate value.

The smaller curve of Fig. 6 exhibits graphically the arrangement of these numbers. As may be seen, the number of repetitions necessary for learning equally long series was a little larger in the earlier period than in the later one. Because of its uniformity this relation is to be attributed to differences in the experimental conditions, to inaccuracies in the calculations, and perhaps also to the increased training of the later period. The older numbers fall very close to the position of the later ones, and—what is of chief importance—the two curves lie as closely together throughout the short extent of their common course as could be desired for tests separated by 3½ years and unaffected by any presuppositions. There is a high degree of probability, then, in favor of the supposition that the relations of dependence presented in those curves, since they remained constant over a long interval of time, are to be considered as characteristic for the person concerned, although they are, to be sure, only individual.

Section 21. Increase in Rapidity of Learning in the Case of Meaningful Material

In order to keep in mind the similarities and differences between sense and nonsense material, I occasionally made tests with the English original of Byron's "Don Juan." These results do not properly belong here since I did not vary the length of the amount to be learned each time but memorised on each occasion only separate stanzas. Nevertheless, it is interesting to mention the number of repetitions necessary because of their contrast with the numerical results just given.

There are only seven tests (1884) to be considered, each of which comprised six stanzas. When the latter, each by itself, were learned to the point of the first possible reproduction, an average of 52 repetitions ($P.E._m = \pm 0.6$) was necessary for all six taken together. Thus, each stanza required hardly nine repetitions; or, if the errorless reproduction is abstracted, scarcely eight repetitions.[1]

[1] For the sake of correct evaluation of the numbers and correct connection with possible individual observations, please note p. 24, 1. In order to procure uniformity of method the stanzas were always read through from beginning to end; more difficult passages were not learned separately and then inserted. If that had been done, the times would have been much shorter and nothing could have been said about

If it is born in mind that each stanza contains 80 syllables (each syllable, however, consisting on the average of less than three letters) and if the number of repetitions here found is compared with the results presented above, there is obtained an approximate numerical expression for the extraordinary advantage which the combined ties of meaning, rhythm, rhyme, and a common language give to material to be memorised. If the above curve is projected in imagination still further along its present course, then it must be supposed that I would have required 70 to 80 repetitions for the memorisation of a series of 80 to 90 nonsense syllables. When the syllables were objectively and subjectively united by the ties just mentioned this requirement was in my case reduced to about one-tenth of that amount.

the number of repetitions. Of course the reading was done at a uniform rate of speed as far as possible, but not in the slow and mechanically regulated time that was employed for the series of syllables. The regulation of speed was left to free estimation. A single reading of one stanza required 20 to 23 seconds.

CHAPTER VI

RETENTION AS A FUNCTION OF THE NUMBER OF REPETITIONS

Section 22. Statement of the Problem

The result of the fourth chapter was as follows: When in repeated cases I memorised series of syllables of a certain length to the point of their first possible reproduction, the times (or number of repetitions) necessary differed greatly from each other, but the mean values derived from them had the character of genuine constants of natural science. Ordinarily, therefore, I learned by heart homogeneous series under similar conditions with, on the average, a similar number of repetitions. The large deviations of the separate values from each other change the total result not at all; but it would require too much time to ascertain with exactness the number necessary for greater precision in detail.

What will happen, it may be asked, if the number of repetitions actually given to a certain series is less than is required for memorisation or if the number exceeds the necessary minimum?

The general nature of what happens has already been described. Naturally the surplus repetitions of the latter alternative do not go to waste. Even though the immediate effect, the smooth and errorless reproduction, is not affected by them, yet they are not without significance in that they serve to make other such reproductions possible at a more or less distant time. The longer a person studies, the longer he retains. And, even in the first case, something evidently occurs even if the repetitions do not suffice for a free reproduction. By them a way is at least opened for the first errorless reproduction, and the disconnected, hesitating, and faulty reproductions keep approximating more and more to it.

These relations can be described figuratively by speaking of the

series as being more or less deeply engraved in some mental substratum. To carry out this figure: as the number of repetitions increases, the series are engraved more and more deeply and indelibly; if the number of repetitions is small, the inscription is but surface deep and only fleeting glimpses of the tracery can be caught; with a somewhat greater number the inscription can, for a time at least, be read at will; as the number of repetitions is still further increased, the deeply cut picture of the series fades out only after ever longer intervals.

What is to be said in case a person is not satisfied with this general statement of a relation of dependence between the number of repetitions and the depth of the mental impression obtained, and if he demands that it be defined more clearly and in detail? The thermometer rises with increasing temperature, the magnetic needle is displaced to an increasing angle as the intensity of the electric current around it increases. But while the mercury always rises by equal spaces for each equal increase in temperature, the increase of the angle showing the displacement of the magnetic needle becomes less with a like increase in the electric current. Which analogy is it which holds for the effect of the number of repetitions of the series to be memorised upon the depth of the resulting impression? Without further discussion shall we make it proportional to the number of repetitions, and accordingly say that it is twice or three times as great when homogenous series are repeated with the same degree of attention twice or thrice as many times as are others? Or does it increase less and less with each and every constant increase in the number of repetitions? Or what does happen?

Evidently this question is a good one; its answer would be of theoretical as well as practical interest and importance. But with the resources hitherto at hand it could not be answered, nor even investigated. Even its meaning will not be quite clear so long as the words "inner stability" and "depth of impression" denote something indefinite and figurative rather than something clear and objectively defined.

Applying the principles developed in section 5, I define the inner stability of a series of ideas—the degree of its retainability—by the greater or less readiness with which it is reproduced at some definite time subsequent to its first memorisation. This readiness I measure by the amount of work saved in the re-

learning of any series as compared with the work necessary for memorising a similar but entirely new series.

The interval of time between the two processes of memorisation is of course a matter of choice. I chose 24 hours.

Since in the case of this definition we are not trying to settle a matter of general linguistic usage, it cannot be properly asked whether it is correct, but only whether it serves the purpose, or, at the most, whether it is applicable to the indefinite ideas connected with the notion of different depths of mental impression. The latter will probably be granted. But nothing can be said in advance as to how well it fulfills its purpose. That can be judged only after more extensive results have been obtained. And the character of the judgment will depend to a great extent on whether the results obtained with the help of this means of measurement fulfill the primary demand which we make with reference to any system of measurement. It consists in this,—that if any change whatever is made in the controllable conditions of that scale, the results obtained by the scale in its new form can be reduced to those of the old form by multiplication by some one constant. In our present case, for example, it would consequently be necessary to know whether the *character* of the results would remain the same if any other interval had been employed instead of that of 24 hours, arbitrarily chosen for measuring the after-effect of repetitions, or whether as a consequence the entire rationale of the results would be different, just as the absolute values are necessarily different. Naturally, this question cannot be decided *a priori*.

For ascertaining the relation of dependence between the increase in the number of repetitions of a series and the ever deeper impression of it which results, I have formulated the problem as follows: If homogeneous series are impressed to different extents as a result of different numbers of repetitions, and then 24 hours later are learned to the point of the first possible reproduction by heart, how are the resulting savings in work related to each other and to the corresponding number of former repetitions?

Section 23. The Tests and their Results

In order to answer the question just formulated, I have carried out 70 double tests, each of six series of 16 syllables each.

Each double test consisted in this, that the separate series—each for itself—were first read attentively a given number of times (after frequently repeated readings they were recited by heart instead of read), and that 24 hours later I relearned up to the point of first possible reproduction the series thus impressed and then in part forgotten. The first reading was repeated 8, 16, 24, 32, 42, 53, or 64 times.

An increase of the readings used for the first learning beyond 64 repetitions proved impracticable, at least for six series of this length. For with this number each test requires about ¾ of an hour, and toward the end of this time exhaustion, headache, and other symptoms were often felt which would have complicated the conditions of the test if the number of repetitions had been increased.

The tests were equally divided among the seven numbers of repetitions investigated so that to each of them were allotted 10 double tests. The results were as follows for the six series of a single test taken together and without subtraction of the time used for reciting.

After a preceding study of the series by means of "x" repetitions, they were learned 24 hours later with an expenditure of "y" seconds.

	x = 8 y =	x = 16 y =	x = 24 y =	x = 32 y =	x = 42 y =	x = 53 y =	x = 64 y =
	1171	998	1013	736	708	615	530
	1070	795	853	764	579	579	483
	1204	936	854	863	734	601	499
	1180	1124	908	850	660	561	464
	1246	1168	1004	892	738	618	412
	1113	1160	1068	868	713	582	419
	1283	1189	979	913	649	572	417
	1141	1186	966	858	634	516	397
	1127	1164	1076	914	788	550	391
	1139	1059	1033	975	763	660	524
m =	1167	1078	975	863	697	585	454
P.E.m =	± 14	± 28	± 17	± 15	± 14	± 9	± 11

The preceding table of numbers gives the times *actually used* in learning by heart the series studied 24 hours previously. Since we are interested not so much in the times used as the times saved, we must know how long it would have taken to learn by heart the same series if no previous study had been made. In the case of the series which were repeated 42, 53, and 64 times,

this time can be learned from the tests themselves. For, in their case, the number of repetitions is greater than the average minimum for the first possible reproduction, which in the case of the 16-syllable series (p. 46) amounted to 31 repetitions. In their case, therefore, the point can be determined at which the first errorless reproduction of that series appeared as the number of repetitions kept on increasing. But on account of the continued increase in the number of repetitions and the resulting extension of the time of the test, the conditions were somewhat different from those in the customary learning of series not hitherto studied. In the case of the series to which a smaller number of repetitions than the above were given, the numbers necessary for comparison cannot be derived from their own records, since, as a part of the plan of the experiment, they were not completely learned by heart. I have consequently preferred each time to find the saving of work in question by comparison with the time required for learning by heart not the same but a similar series up to that time unknown. For this I possess a fairly correct numerical value from the time of the tests in question: any six 16-syllable series was learned, as an average of 53 tests, in 1,270 seconds, with the small probable error ± 7.

If all the mean values are brought together in relation to this last value, the following table results:

I After a preceding study of the series by X repetitions,	II They were just memorized 24 hours later in Y seconds		III The result therefore of the preceding study was a saving of T seconds,		IV Or, for each of the repetitions, an average saving of D seconds
X=	Y=	P.E.$_m$=	T=	P.E.$_m$=	D=
0	1270	7			
8	1167	14	103	16	12.9
16	1078	28	192	29	12.0
24	975	17	295	19	12.3
32	863	15	407	17	12.7
42	697	14	573	16	13.6
53	585	9	685	11	12.9
64	454	11	816	13	12.8
					m= 12.7

The simple relation approximately realised in these numbers is evident: the number of repetitions used to impress the series (Column I) and the saving in work in learning the series 24 hours later as a result of such impression (Col. III) increase in the same fashion. Division of the amount of work saved by the corresponding number of repetitions gives as a quotient a practically constant value (Col. IV).

Consequently the results of the test may be summarised and formulated as follows: When nonsense series of 16 syllables each were impressed in memory to greater and greater degrees by means of attentive repetitions, the inner depth of impression in part resulting from the number of the repetitions increased, within certain limits, approximately proportionally to that number. This increase in depth was measured by the greater readiness with which these series were brought to the point of reproduction after 24 hours. The limits within which this relation was determined were on the one side, zero, and, on the other, about double the number of repetitions that on the average just sufficed for learning the series.

For six series taken together the after-effect of each repetition—*i.e.*, the saving it brought about—amounted on the average to 12.7 seconds, consequently to 2.1 seconds for each single series. As the repetition of a series of 16 syllables in itself takes from 6.6 to 6.8 seconds, its after-effect 24 hours later amounts to a scant third of its own duration. In other words: for each three additional repetitions which I spent on a given day on the study of a series, I saved, in learning that series 24 hours later, on the average, approximately one repetition; and, within the limits stated, it did not matter how many repetitions altogether were spent on the memorisation of a series.

Whether the results found can claim any more general importance, or whether they hold good only for the single time of their actual occurrence, and even then give a false impression of a regularity not otherwise present, I cannot now decide. I have no direct control tests. Later, however, (chapter VIII, § 34) where results obtained in reference to quite a different problem agree with the present results, I can bring forward indirect evidence on this point. I am therefore inclined to

ascribe general validity to these results, at least for my own case.

NOTE.—There is in the tests an inner inequality which I can neither avoid, nor remove by correction, but can only point out. It is that a small number of repetitions of the series requires only a few minutes, and consequently come at a time of unusual mental vitality. With 64 repetitions the whole work takes about ¾ of an hour; the great part of the series is, therefore, studied in a condition of diminished vigor or even of a certain exhaustion, and the repetitions will, consequently, be less effective. It is just the reverse of this in the reproduction of the series the next day. The series impressed by 8 perusals require three times as much time in order to be memorised as those perused 64 times. Consequently the latter will be learned a little more quickly not only on account of their greater fixedness, but also because they are now studied for the most part under better conditions. These irregularities are mutually opposed, as is evident, and therefore partially compensate each other: the series prepared under comparatively unfavorable conditions are memorised under comparatively more favorable conditions, and vice versa. I cannot tell, however, how far this compensation goes and how far any remaining inequality of conditions disturbs the results.

Section 24. The Influence of Recollection

One factor in the regular course of the results obtained seems to deserve special attention. In ordinary life it is of the greatest importance, as far as the form which memory assumes is concerned, whether the reproductions occur with accompanying recollection or not,—*i.e.*, whether the recurring ideas simply return or whether a knowledge of their former existence and circumstances comes back with them. For, in this second case, they obtain a higher and special value for our practical aims and for the manifestations of higher mental life. The question now is, what connection is there between the inner life of these ideas and the complicated phenomena of recollection which sometimes do and sometimes do not accompany the appearance in consciousness of images? Our results contribute somewhat toward the answer to this question.

When the series were repeated 8 or 16 times they had become unfamiliar to me by the next day. Of course, indirectly, I knew quite well that they must be the same as the ones studied the day before, but I knew this only indirectly. I did not get it from the series, I did not recognise them. But with 53 or 64 repetitions I soon, if not immediately, treated them as old acquaintances, I remembered them distinctly. Nothing corresponding to this difference is evident in the times for memorisation and for savings of work respectively. They are not smaller *relatively*

when there is no possibility of recollection nor larger *relatively* when recollection is sure and vivid. The regularity of the after-effect of many repetitions does not noticeably deviate from the line that is, so to speak, marked out by a smaller number of repetitions although the occurrence of this after-effect is accompanied by recollection in the first case just as indubitably as it lacks recollection in the second case.

I restrict myself to pointing out this noteworthy fact. General conclusions from it would lack foundation as long as the common cause cannot be proved.

Section 25. The Effect of a Decided Increase in the Number of Repetitions

It would be of interest to know whether the approximate proportionality between the number of repetitions of a series and the saving of the work in relearning the latter made possible thereby, which in my own case seemed to take place within certain limits, continues to exist beyond those limits. If, furthermore, as a result of each repetition a scant third of its own value is saved up to be applied on the reproduction 24 hours later, I should be able to just reproduce spontaneously after 24 hours a series of 16 syllables, the initial syllable being given, provided I had repeated it the first day thrice as many times as were absolutely necessary for its first reproduction. As this requirement is 31-32 repetitions the attainment of the aim in question would necessitate about 100 repetitions. On the supposition of the general validity of the relation found, the number of repetitions to be made at a given time, in order that errorless reproduction might take place 24 hours later, could be calculated for any kind of series for which, so to say, the "after-effect-coefficient" of the repetitions had been ascertained.

I have not investigated this question by further increasing the number of repetitions of unfamiliar 16 syllable series because, as has been already noted, with any great extension of the tests the increasing fatigue and a certain drowsiness cause complications. However, I have made some trial tests partly with shorter series, and partly with familiar series, all of which confirmed the result that the proportion in question gradually ceases to hold with a further increase of repetitions. Measured by the saving

of work after 24 hours the effect of the later repetitions gradually decreases.

Series of 12 syllables (six of the series were each time combined into a test) were studied to the point of first possible reproduction; and immediately after the errorless reproduction each series was repeated three times (in all four times) as often as the memorisation (exclusive of the recital) had required. After 24 hours the same series were relearned to the first possible reproduction. Four tests furnished the following results (the numbers indicate the repetitions):

Repetitions for the learning and recital of 6 series	Immediately successive repetitions for the sake of greater surety	Total number of repetitions used for the 6 series	After 24 hours the memorization of the series required	Thus the work saved by the total number of repetitions amounted to
104	294	398	41	63
101	285	386	39	62
114	324	438	46	68
109	309	418	38	71
$m =$ 107	303	410	41	66 $P.E._m = 1.4$

In my own case—within reasonable limits—the after-effect of the repetitions of series of 12 syllables after 24 hours is a little smaller than is the case with 16 syllables; it must be estimated as at least three tenths of the sum total of the repetitions. If this relation were approximately to continue to hold with very numerous repetitions, it would be reasonable to expect that, after 24 hours, series on whose impression four times as many repetitions had been expended as were necessary for their first reproduction could be recited without any further expenditure of energy. Instead of this, in the cases examined, the relearning required about 35 per cent of the work required for the first recital. The effect of an average number of 410 repetitions was a saving of only one sixth of this sum. If now the first repetitions were represented by about three tenths of their amount, the effect of the later repetitions must have been very slight.

Investigations of the following kind, which I do not here give in detail, led to the same result. Syllable series of different

lengths were gradually memorised by frequent repetitions which however, did not all take place on one day, but were distributed over several successive days (Chap. VIII). When, after several days, only a few repetitions were necessary in order to learn the series by heart, they were repeated three or four times as often as was necessary, at this phase of memorisation, for the first errorless reproduction. But in no single case did I succeed in an errorless reproduction of the series after 24 hours unless I had read them again once or several times. The influence of the frequent repetitions still appeared, indeed, in a certain saving of work, but this became less in proportion to the decreasing amount of work to be saved. It was very hard, by means of repetitions which had taken place 24 hours previously, to eliminate the last remnant of the work of relearning a given series.

To summarise: The effect of increasing the number of repetitions of series of syllables on their inner fixedness in the above defined sense grew at first approximately in proportion to the number of repetitions, then that effect decreased gradually, and finally became very slight when the series were so deeply impressed that they could be repeated after 24 hours, almost spontaneously. Since this decrease must be considered gradual and continuous, its beginning would, in more accurate investigations, probably have become evident even within the limits within which we found a proportionality, whereas now it is hidden on account of its small amount and the wide limits of error.

CHAPTER VII

RETENTION AND OBLIVISCENCE AS A FUNCTION OF THE TIME

Section 26. Explanations of Retention and Obliviscence

All sorts of ideas, if left to themselves, are gradually forgotten. This fact is generally known. Groups or series of ideas which at first we could easily recollect or which recurred frequently of their own accord and in lively colors, gradually return more rarely and in paler colors, and can be reproduced by voluntary effort only with difficulty and in part. After a longer period even this fails, except, to be sure, in rare instances. Names, faces, bits of knowledge and experience that had seemed lost for years suddenly appear before the mind, especially in dreams, with every detail present and in great vividness; and it is hard to see whence they came and how they managed to keep hidden so well in the meantime. Psychologists—each in accordance with his general standpoint—interpret these facts from different points of view, which do not exclude each other entirely but still do not quite harmonise. One set, it seems, lays most importance on the remarkable recurrence of vivid images even after long periods. They suppose that of the perceptions caused by external impressions there remain pale images, "traces," which, although in every respect weaker and more flighty than the original perceptions, continue to exist unchanged in the intensity possessed at present. These mental images cannot compete with the much more intense and compact perceptions of real life; but where the latter are missing entirely or partly, the former domineer all the more unrestrainedly. It is also true that the earlier images are more and more overlaid, so to speak, and covered by the later ones. Therefore, in the case of the earlier images, the possibility of recurrence offers itself more rarely and with greater difficulty. But if, by an accidental and favorable grouping of circumstances, the accumu-

lated layers are pushed to one side, then, of course, that which was hidden beneath must appear, after whatever lapse of time, in its original and still existent vividness.[1]

For others[2] the ideas, the persisting images, suffer changes which more and more affect their nature; the concept of obscuration comes in here. Older ideas are repressed and forced to sink down, so to speak, by the more recent ones. As time passes one of these general qualities, inner clearness and intensity of consciousness, suffers damage. Connections of ideas and series of ideas are subject to the same process of progressive weakening; it is furthered by a resolution of the ideas into their components, as a result of which the now but loosely connected members are eventually united in new combinations. The complete disappearance of the more and more repressed ideas occurs only after a long time. But one should not imagine the repressed ideas in their time of obscuration to be pale images, but rather to be tendencies, "dispositions," to recreate the image contents forced to sink down. If these dispositions are somehow supported and strengthened, it may happen at any one moment that

[1] This is the opinion of Aristotle and is still authoritative for many people. Lately, for instance, Delboeuf has taken it up again, and has used it as a complement to his "théorie générale de la sensibilité." In his article, *Le sommeil et les rêves* (*Rev. Philos.* IX, p. 153 f.), he says: "Nous voyons maintenant que tout acte de sentiment, de pensée ou de volition en vertu d'une loi universelle imprime en nous une trace plus ou moins profonde, mais indélébile, généralement gravée sur une infinité de traits antérieurs, surchargée plus tard d'une autre infinité de linéaments de toute nature, mais dont l'écriture est néanmoins indéfiniment susceptible de reparaitre vive et nette au jour." (We see now that by a general law every act of feeling, thought or will leaves a more or less deep but indelible impress upon our mind, that such a tracing is usually graven upon an infinite number of previous traces and later is itself overlaid with innumerable others but nevertheless is still capable of vivid and clear reappearance.) It is true that he proceeds: "néanmoins . . . il y a quelque vérité dans l'opinion qui veut que la mémoire non seulement se fatigue mais s'oblitère" (nevertheless . . . there is some truth in the opinion that memory not only becomes fatigued but that it disappears"), but he explains this by the theory that one memory might hinder another from appearing. "Si un souvenir ne chasse pas l'autre on peut du moins prétendre qu'un souvenir empêche l'autre et qu'ainsi pour la substance cérébrale, chez l'individu, il y a un maximum de saturation." (If one recollection does not actually drive out another, it may at least be maintained that one recollection hinders the other and that thus the brain of each individual is saturated.)

The curious theory of Bain and others that each idea is lodged in a separate ganglion cell, an hypothesis impossible both psychologically and physiologically, is also rooted to a certain extent in Aristotle's view.

[2] Herbart and his adherents. See, for instance, Waitz, Lehrbuch der Psychologie Sect. 16.

the repressing and hindering ideas become depressed themselves, and that the apparently forgotten idea arises again in perfect clearness.

A third view holds that, at least in the case of complex ideas, obliviscence consists in the crumbling into parts and the loss of separate components instead of in general obscuration. The idea of resolution into component parts recently spoken of supplies here the only explanation. "The image of a complex object is dim in our memory ~~not because~~ as a whole with all its parts present and in order it is illuminated by a feebler light of consciousness, but it is because it has become incomplete. Some parts of it are entirely lacking. Above all the precise connection of those still extant is, in general, missing and is supplied only by the thought that some sort of union once existed between them; the largeness of the sphere in which, without being able to make a final decision, we think this or that connection equally probable, determines the degree of dimness which we are to ascribe to the idea in question."[1]

Each of these opinions receives a certain, but not exclusive, support from the actual inner experiences, or experiences supposed to be actual, which we at times have. And what is the reason? It is that these fortuitous and easily obtained inner experiences are much too vague, superficial and capable of various interpretations to admit in their entirety of only a single interpretation, or even to let it appear as of preponderating probability. Who could, with even tolerable exactness, describe in its gradual course the supposed overlaying or sinking or crumbling of ideas? Who can say anything satisfactory about the inhibitions caused by series of ideas of different extent, or about the disintegration that a firm complex of any kind suffers by the use of its components in new connections? Everybody has his private "explanation" of these processes, but the actual conditions which are to be explained are, after all, equally unknown to all of us.

If one considers the limitation to direct, unaided observation and to the chance occurrence of useful experiences, there seems but little prospect of improvement in conditions. How will one for example determine the degree of obscuration reached at a certain point, or the number of fragments remaining? Or

[1] Lotze, Metaphysik (1879), p. 521; also Mikrokosmos (3) I, p. 231 ff.

how can the probable course of inner processes be traced if the almost entirely forgotten ideas return no more to consciousness?

Section 27. Methods of Investigation of Actual Conditions

By the help of our method we have a possibility of indirectly approaching the problem just stated in a small and definitely limited sphere, and, by means of keeping aloof for a while from any theory, perhaps of constructing one.

After a definite time, the hidden but yet existent dispositions laid down by the learning of a syllable-series may be strengthened by a further memorisation of the series, and thereby the remaining fragments may be united again to a whole. The work necessary for this compared with that necessary when such dispositions and fragments are absent gives a measure for what has been lost as well as for what remains. The inhibitions which idea-groups of different sorts or extents may occasion in relation to others must, as a result of the interposition of well defined complexes of ideas between learning and relearning, betray itself in the more or less increased work of relearning. The loosening of a bond of connection by some other use of its components can be investigated in a similar manner as follows: after a certain series has been studied, new combinations of the same series are memorised and the change in the amount of work necessary for relearning the original combination is then ascertained.

First, I investigated the first mentioned of these relations and put the question: If syllable series of a definite kind are learned by heart and then left to themselves, how will the process of forgetting go on when left merely to the influence of time or the daily events of life which fill it? The determination of the losses suffered was made in the way described: after certain intervals of time, the series memorised were relearned, and the times necessary in both cases were compared.

The investigations in question fell in the year 1879-80 and comprised 163 double tests. Each double test consisted in learning eight series of 13 syllables each (with the exception of 38 double tests taken from 11-12 A. M. which contained only six series each) and then in relearning them after a definite time. The learning was continued until two errorless recitations of the series in question were possible. The relearning was carried to the same point; it occurred at one of the following seven

times,—namely, after about one third of an hour, after 1 hour, after 9 hours, one day, two days, six days, or 31 days.

The times were measured from the completion of the first set of first learnings, as a consequence of which no great accuracy was required in case of the longer intervals. The influence of the last four intervals was tested at three different times of day (p. 33). Some preliminary remarks are necessary before the results obtained can be communicated.

Similar experimental conditions may be taken for granted in the case of the series relearned after a number of whole days. At any rate there is no way of meeting the actual fluctuations even when external conditions are as far as possible similar, other than by a multiplication of the tests. Where the inner dissimilarity was presumably the greatest, namely after the interval of an entire month, I approximately doubled the number of tests.

In the case of an interval of nine hours and an interval of one hour between learning and relearning, there existed, however, a noticeably constant difference in the experimental conditions. In the later hours of the day mental vigor and receptivity are less. The series learned in the morning and then relearned at a later hour, aside from other influences, require more work for relearning than they would if the relearning were done at a time of mental vigor equal to that of the original learning. Therefore, in order to become comparable, the numerical values found for relearning must suffer a diminution which, at least in the case of the 8 hour interval, is so considerable that it cannot be neglected. It must be ascertained how much longer it takes to learn at the time of day, B, series which were learned in "a" seconds at the time of day, A. The actual determination of this quantity presupposes more tests than I, up to the present, possess. If a necessary but inexact correction is applied to the numbers found for 1 and for 8 hours, these become even more unreliable than if left to themselves.

In the case of the smallest interval, one third of an hour, the same drawback reappears, though to a less degree; but it is probably compensated for by another circumstance. The interval as a whole is so short that the relearning of the first series of a test followed almost immediately or after an interval of one or two minutes upon the learning of the last series of the same test. For this reason the whole formed, so to speak, one con-

tinuous test in which the relearning of the series took place under increasingly unfavorable conditions as regards mental freshness. But on the other hand the relearning after such a short interval was done rather quickly. It took hardly half of the time required for the learning. By this means the interval between the learning and relearning of a certain series became gradually smaller. The later series therefore had more favorable conditions with regard to the time interval. In view of the difficulty of more accurate determinations, I have taken it for granted that these two supposed counteracting influences approximately compensated each other.

Section 28. Results

In the following table I denote by:

L the time of first learning of the series in seconds, just as they were found, therefore including the time for the two recitations.

WL the time for relearning the series also including the recitations.

WLk the time of relearning reduced where necessary by a correction.

Δ the difference L—WL or L—WLk, as the case may be—that is, the saving of work in the case of relearning.

Q the relation of this saving of work to the time necessary for the first learning, given as a per cent. In the calculation of this quotient I considered only the actual learning time, the time for recitation having been subtracted.[1]

The latter was estimated as being 85 seconds for two recitations of 8 series of 13 syllables each; that would correspond to

[1] A theoretically correct determination of the Probable Errors of the differences and quotients found would be very difficult and troublesome. The directly observed values L and WL would have to be made the basis of it. But the ordinary rules of the theory of errors cannot be applied to these values, because these rules are valid only for observations gained independently of one another, whereas L and WL are inwardly connected because they were obtained from the same series. The source of error, "difficulty of the series," does not vary by chance, but in the same way for each pair of values. Therefore I took here the learning and relearning of the series as a single test and the resulting Δ or Q, as the case may be, as its numerical representative. From the independently calculated Δ and Q, the probable errors were then calculated just as from directly observed values. That is sufficient for an approximate estimate of the reliability of the numbers.

a duration of 0.41 seconds for each syllable (p. 31).

Thus $Q = \frac{100\ \Delta}{L-85}$

Finally A, B, C mean the previously mentioned times of day, 10-11 A. M., 11-12 A. M., 6-8 P. M.

I. 19 minutes. 12 tests. Learning and relearning at the time A.

L	WL	Δ	Q
1156	467	689	64.3
1089	528	561	55.9
1022	492	530	56.6
1146	483	663	62.5
1115	490	625	60.7
1066	447	619	63.1
985	453	532	59.1
1066	517	549	56.0
1364	540	824	64.4
975	577	398	44.7
1039	528	511	53.6
952	452	500	57.7
m=1081	498	583	58.2 P.E.$_m$=1

II. 63 minutes. 16 tests. The learning at the time A, the relearning at the time B. For ascertaining the influence of this difference in the time of day I have the following data. Six series of 13 syllables each were learned at the time B (not including the time of recitation) in 807 seconds (P.E.$_m$=10) as an average of 39 tests. Just as many series of the same kind were learned at the time A in 763 seconds (P.E.$_m$=7) as an average of 92 tests. Thus the values found in the later period are about 5 per cent. of their own amount too large as compared with those obtained in the earlier period. Therefore the times found for relearning at the time B must be decreased by about 5 per cent. to make them comparable with those of learning.

L	WL	WLk	Δ	Q
1095	625	594	501	49.6
1195	821	780	415	37.4
1133	669	636	497	47.4
1153	687	653	500	46.8
1134	626	595	539	51.4
1075	620	589	486	49.1
1138	704	669	469	44.5
1078	565	537	541	54.5
1205	770	731	474	42.3
1104	723	687	417	40.9
886	644	612	274	34.2
958	591	562	396	45.4
1046	739	702	344	35.8
1122	790	750	372	35.9
1100	609	579	521	51.3
1269	709	674	595	50.0
m 1106	681	647	459	44.2 P.E.$_m$=1

III. 525 minutes. 12 tests. The learning at the time A, the relearning at the time C. The different influence of the two times of day is calculated as follows: eight series of 13 syllables each required, with 38 tests at time C, 1173 seconds ($P.E._m = 10$); similar series with 92 tests at time A required 1027 seconds ($P.E._m = 8$). The first number is approximately 12 per cent. of its own value larger than the second; therefore I have subtracted that much from the numerical values found for the time C.

	L	WL	WLk	Δ	Q
	1219	921	811	408	36.0
	975	815	717	258	29.0
	1015	858	755	260	28.0
	954	784	690	264	30.4
	1340	955	840	500	39.8
	1061	811	714	347	35.6
	1252	784	690	562	48.2
	1067	860	757	310	31.6
	1343	1019	897	446	35.5
	1181	842	741	440	40.1
	1080	799	703	377	37.9
	1091	806	709	382	38.0
m	1132	855	752	380	35.8 $P.E._m = 1$

IV. One day. 26 tests, of which 10 were at time A, 8 at time B (here as everywhere consisting of only 6 series each), 8 at time C.

A

	L	WL	Δ	Q
	1072	811	261	26.4
	1369	861	508	39.6
	1227	823	404	35.4
	1263	793	470	39.9
	1113	754	359	34.9
	1000	644	356	38.9
	1103	628	475	46.7
	888	754	134	16.7
	1030	829	201	21.3
	1021	660	361	38.6
m	1109	756	353	33.8 $P.E._m = 2$

B

L	WL	Δ	Q
889	650	239	29.0
824	537	287	37.8
897	593	304	36.5
825	599	226	29.7
854	562	292	37.0
863	761	122	14.9
742	433	309	45.6
907	653	254	30.1
m 853	599	254	32.6
			$P.E._m = 2.2$

C

L	WL	Δ	Q
1212	935	277	24.6
1215	797	418	37.0
1096	647	449	44.4
1191	684	507	45.8
1256	898	358	30.6
1295	781	514	42.5
1146	936	210	19.8
1064	750	314	32.1
m 1184	803	381	34.6
			$P.E._m = 2.3$

The average differences between times of learning and relearning at the different times of day vary somewhat with regard to their absolute values. (Of course in the case of B the number 254 must first be multiplied by 4/3, because it is derived from only 6 series.) But the relations of these differences to the times of first learning (the Q's) harmonize sufficiently well. If, therefore, all the Q's are combined in a general average, Q=33.7 (P.E.$_m$=1.2).

V. Two days. 26 tests; 11 of these at the time A, 7 at B, 8 at C.

A

L	WL	Δ	Q
1066	895	171	17.4
1314	912	402	32.7
963	855	108	12.3
964	710	254	28.9
1242	888	354	30.6
1243	710	533	46.0
1144	895	249	23.5
1143	874	269	25.4
1149	953	196	18.4
1090	855	235	23.4
1376	847	529	41.0
m 1154	854	300	27.2
			P.E.$_m$=2.3

B

L	WL	Δ	Q
752	549	203	29.5
1087	740	347	33.9
1073	620	453	44.9
826	693	133	17.5
905	548	357	42.4
811	763	48	6.4
782	618	164	22.8
m 891	647	244	28.2
			P.E.$_m$=3.5

C

L	WL	Δ	Q
1246	889	357	31.6
1231	885	346	30.2
1273	1039	234	19.7
1319	925	394	31.9
1125	971	154	14.8
1275	891	384	32.3
1322	857	465	37.6
1170	880	290	26.7
m 1245	917	328	28.1
			$P.E._m=1.8$

The combination of the three average values of Q, which are stated in per cents and lie close to each other, for the 26 tests gives $Q=27.8$ ($P.E._m=1.4$)

VI. Six days. 26 tests, 10 at the time A, 8 at B, 8 at C.

A

L	WL	Δ	Q
1076	868	208	21.0
992	710	282	31.1
1082	756	326	32.7
1260	973	287	24.4
1032	864	168	17.7
1010	955	55	5.9
1197	818	379	34.1
1199	828	371	33.3
943	697	246	28.7
1105	868	237	23.2
m 1090	834	260	25.2
			$P.E._m=1.9$

B

	L	WL	Δ	Q
	902	564	338	40.3
	793	517	276	37.9
	848	639	209	26.5
	871	709	162	20.1
	1034	649	385	39.7
	745	728	17	2.5
	975	645	330	36.2
	805	766	39	5.3
m	872	652	220	26.1
				$P.E._m=4$

C

	L	WL	Δ	Q
	1246	922	324	27.9
	1334	1097	237	19.0
	1293	939	354	21.0
	1401	988	413	31.4
	1214	992	222	19.7
	1299	1045	254	20.9
	1358	1047	311	24.4
	1305	881	424	34.8
m	1306	989	317	24.9
				$P.E._m=1.6$

The average of the total 26 savings of work, stated in per cents, is 25.4 ($P.E._m$=1.3).

VII. 31 days. 45 tests; 20 at the time A, 15 at B, 10 at C.

A

	L	WL	Δ	Q
	1069	813	256	26.0
	1109	785	324	31.6
	1268	858	410	34.7
	1280	902	378	31.6
	1180	848	332	30.3
	1095	888	207	20.5
	1089	988	101	10.1
	1113	1043	70	6.8
	1090	1025	65	6.5
	997	876	121	13.3
	1116	934	182	17.7
	1060	893	167	17.1
	930	796	134	15.9
	1030	769	261	27.6
	980	862	118	13.2
	1079	805	274	27.6
	1254	978	276	23.6
	1164	938	226	20.9
	1127	869	258	24.8
	1268	972	296	25.0
m	1115	892	223	21.2
				$P.E._m$=1.3

B

	L	WL	Δ	Q
	831	638	193	25.2
	867	516	351	43.7
	960	748	212	23.7
	828	675	153	20.0
	859	705	154	19.4
	838	661	177	22.9
	946	887	59	6.7
	833	780	53	6.9
	696	532	164	25.9
	757	626	131	18.9
	906	733	173	20.5
	1024	915	109	11.4
	930	780	150	17.3
	899	756	143	17.1
	1018	705	313	32.8
m	879	710	169	20.8
				$P.E._m$=1.4

C

L	WL	Δ	Q
1424	1004	420	31.4
1307	1102	205	16.4
1351	893	458	36.2
1245	1090	155	13.4
1258	895	363	31.0
1155	1070	85	7.9
1219	800	419	36.9
1278	1110	168	14.1
1120	1051	69	6.7
1250	1055	195	16.7
m 1261	1007	254	21.1
			$P.E._m = 2.7$

The average of 45 savings of work expressed in per cents=21.1 ($P.E._m = 0.8$)

A hasty glance at the figures above reveals the fact that for each interval of time the savings in work which become evident when the series is relearned have very fluctuating values. (This saving in work is each time the measure for the amount remembered at the end of the interval.) This is especially the case with their absolute values (Δ), but is also the case with the relative values (Q). The results are taken from the earlier period and suffer from several disturbing influences to which my attention was first drawn by the tests themselves.

In spite of all irregularities in detail, however, they group themselves as a whole with satisfactory certainty into an harmonious picture. As a proof of this the absolute amount of the saving in work is of less value. The latter evidently depends upon the time of day—*i.e.*, upon the changes in the time of the first learning dependent upon it. When this change is greatest (time C), Δ also is greatest; for the time B, they are in ¾ of the cases larger than for the time A (after multiplying by 4/3). On the other hand, the values (Q) found for the relation of each saving of work to the time originally spent, are apparently almost independent of this ratio. Their averages are close together for all three times of day, and do not show any character of increase or decrease in the later hours. Accordingly I here tabulate the latter.

No.	I After X hours	II So much of the series learned was retained that in relearning a saving of Q % of the time of original learning was made	III P.E.$_m$	IV The amount forgotten was thus equivalent to v % of the original in terms of time of learning
	X=	Q=		v=
1	0.33	58.2	1	41.8
2	1.	44.2	1	55.8
3	8.8	35.8	1	64.2
4	24.	33.7	1.2	66.3
5	48.	27.8	1.4	72.2
6	6 x 24	25.4	1.3	74.6
7	31 x 24	21.1	0.8	78.9

Section 29. Discussion of Results

1. It will probably be claimed that the fact that forgetting would be very rapid at the beginning of the process and very slow at the end should have been foreseen. However, it would be just as reasonable to be surprised at this initial rapidity and later slowness as they come to light here under the definite conditions of our experiment for a certain individual, and for a series of 13 syllables. One hour after the end of the learning, the forgetting had already progressed so far that one half the amount of the original work had to be expended before the series could ~~be reproduced~~ again; after 8 hours the work to be made up amounted to two thirds of the first effort. Gradually, however, the process became slower so that even for rather long periods the additional loss could be ascertained only with difficulty. After 24 hours about one third was always remembered; after 6 days about one fourth, and after a whole month fully one fifth of the first work persisted in effect. The decrease of this after-effect in the latter intervals of time is evidently so slow that it is easy to predict that a complete vanishing of the effect of the first memorisation of these series would, if they had been left to themselves, have occurred only after an indefinitely long period of time.

2. Least satisfactory in the results is the difference between the third and fourth values, especially when taken in connection with the greater difference between the fourth and fifth numbers. In the period 9-24 hours the decrease of the after-effect would

accordingly have been 2½ per cent. In the period 24 to 48 hours it would have been 6.1 per cent; in the later 24 hours, then, about three times as much as in the earlier 15. Such a condition is not credible, since in the case of all the other numbers the decrease in the after-effect is greatly retarded by an increase in time. It does not become credible even under the plausible assumption that night and sleep, which form a greater part of the 15 hours but a smaller part of the 24, retard considerably the decrease in the after-effect.

Therefore it must be assumed that one of these three values is greatly affected by accidental influences. It would fit in well with the other observations to consider the number 33.7 per cent for the relearning after 24 hours as somewhat too large and to suppose that with a more accurate repetition of the tests it would be 1 to 2 units smaller. However, it is upheld by observations to be stated presently, so that I am in doubt about it.

3. Considering the special, individual, and uncertain character of our numerical results no one will desire at once to know what "law" is revealed in them. However, it is noteworthy that all the seven values which cover intervals of one third of an hour in length to 31 days in length (thus from singlefold to 2,000fold) may with tolerable approximation be put into a rather simple mathematical formula. I call:

t the time in minutes counting from one minute before the end of the learning,

b the saving of work evident in relearning, the equivalent of the amount remembered from the first learning expressed in percentage of the time necessary for this first learning,

c and k two constants to be defined presently

Then the following formula may be written:

$$b = \frac{100\,k}{(\log t)^c + k}$$

By using common logarithms and with merely approximate estimates, not involving exact calculation by the method of least squares,

$k = 1.84$

$c = 1.25$

Then the results are as follows:

t	b Observed	b Calculated	Δ
20	58.2	57.0	+ 1.2
64	44.2	46.7	— 2.5
526	35.8	34.5	+ 1.3
1440	33.7	30.4	+ 3.3
2 x 1440	27.8	28.1	— 0.3
6 x 1440	25.4	24.9	+ 0.5
31 x 1440	21.1	21.2	— 0.1

The deviations of the calculated values from the observed values surpass the probable limits of error only at the second and fourth values. With regard to the latter I have already expressed the conjecture that the test might have given here too large a value; the second suffers from an uncertainty concerning the correction made. By the determination made for t, the formula has the advantage that it is valid for the moment in which the learning ceases and that it gives correctly $b = 100$. In the moment when the series can just be recited, the relearning, of course, requires no time, so that the saving is equal to the work expended.

Solving the formula for k we have

$$k = \frac{b\,(\log t)^c}{100 - b}$$

This expression, $100 - b$ the complement of the work saved, is nothing other than the work required for relearning, the equivalent of the amount forgotten from the first learning. Calling this, v, the following simple relation results:

$$\frac{b}{v} = \frac{k}{(\log t)^c}$$

To express it in words: when nonsense series of 13 syllables each were memorised and relearned after different intervals, the quotients of the work saved and the work required were about inversely proportional to a small power of the logarithm of those intervals of time. To express it more briefly and less accurately: the quotients of the amounts retained and the amounts forgotten were inversely as the logarithms of the times.

Of course this statement and the formula upon which it rests have here, no other value than that of a shorthand statement of the above results which have been found but once and under the circumstances described. Whether they possess a more general significance so that, under other circumstances or with other individuals, they might find expression in other constants I cannot at the present time say.

Section 30. Control Tests

At any rate, even though only for my own case, I can to a certain extent give support to two of the values mentioned by tests which were made at other periods.

From a period even further back than that of the investigations above mentioned I possess several tests with series of ten syllables, fifteen series composing one test. The series were first memorised and then, at an average of 18 minutes after the first learning, each series was relearned. Six tests had the following results:—

L	LW	Δ	Q*
848	436	412	57.5
963	535	428	50.9
921	454	467	58.5
879	444	435	57.5
912	443	469	59.4
821	461	360	51.6
m = 891	462	429	56.0 $P.E._m = 1$

* The time subtracted from the L when calculating the Q for two reproductions of 15 series is 123 seconds.

When relearning series of ten syllables each, 18 minutes after the first memorisation, 56 per cent of the work originally expended was therefore saved. The number agrees satisfactorily with the one found above (p. 68) for the relearning of series of syllables of 13 syllables each after 19 minutes, 58 per cent. Also the fact that the latter, notwithstanding the longer interval, is still a little greater, harmonises completely, as will be seen, with the results of the next chapter. According to them, shorter series, when memorised, are forgotten a little more quickly than longer ones.

From the period of 1883-84, I have seven tests, consisting of nine series of 12 syllables each that were relearned 24 hours after the first memorisation. The following results were obtained:

	L	LW	Δ	Q
	791	508	283	37.9
	750	522	228	32.3
	911	533	378	43.6
	725	494	231	33.9
	783	593	190	27.1
	879	585	294	35.2
	689	535	154	23.9
m	790	539	251	33.4
				P.E.$_m$=1.7

The after-effect of the first memorisation still noticeable after 24 hours, was here equivalent to a saving of work of 33.4 per cent of the first expenditure. This number also agrees satisfactorily with the one communicated above for the relearning of series of 13 syllables each after 24 hours (33.7 per cent), although these two were obtained at far separated time-periods and in the course of widely different investigations.

CHAPTER VIII

RETENTION AS A FUNCTION OF REPEATED LEARNING

Section 31. Statement of the Problem and the Investigation

Series of syllables which have been learned by heart, forgotten, and learned anew must be similar as to their inner conditions at the times when they can be recited. The energy of the ideational activity which is directed upon them and which serves to establish them is in both cases so far heightened that quite similar combinations of movements occur in connection with them. For the period after the recital this inner similarity ceases. The series are gradually forgotten, but—as is sufficiently well known—the series which have been learned twice fade away much more slowly than those which have been learned but once. If the relearning is performed a second, a third or a greater number of times, the series are more deeply engraved and fade out less easily and finally, as one would anticipate, they become possessions of the soul as constantly available as other image-series which may be meaningful and useful.

I have attempted to obtain numerical data on the relation of dependence which exists between the permanence of retention of a series and the number of times it has been brought, by means of renewed learning, to a just possible reproduction. The relation is quite similar to that described in Chapter VI as existing between the surety of the series and the number of its repetitions. In the present case, however, the repetitions do not take place all at once, but at separate times and in ever decreasing frequency. On account of our limited insight into the inner connection of these processes we would not be justified in venturing an assertion about one relation on the basis of the other.

Only one value of the time interval between the separate relearnings was chosen, namely, 24 hours. Instead of changing intervals, series of different lengths were chosen for the investi-

gation, the lengths being 12, 24 and 36 syllables. A single test consisted of nine series of the first length, or three of the second, or two of the third. In addition to this I carried out several tests with six stanzas of Byron's "Don Juan."

The plan of the experiment was, then, as follows: The required number of series was first learned and then, at the same hour on successive days, it was relearned to the point of first possible reproduction. In the case of the series of syllables, the number of days was six; in the case of Byron's stanzas, it was only four. Thus, on the fifth day, the stanzas were correctly repeated without any preliminary reproduction and the problem, accordingly, no longer existed. For each kind of series, seven trials were employed. The total number of separate tests was, in consequence, 154, a number of which required only a few minutes for their execution.

The entries of the following tables indicate the repetitions which were necessary in order to bring the series concerned to the first possible reproduction (including this); the Roman figures designate the successive days.

1. Nine series of 12 syllables each.

I	II	III	IV	V	VI
158	102	71	50	38	30
151	107	74	42	34	30
175	105	84	60	36	33
149	102	72	54	35	28
163	124	69	61	35	31
173	117	86	64	42	37
138	106	71	59	37	30
m 158	109	75	56	37	31
P.E.$_m$ 3.4	2	1.7	2	0.7	0.7

2. Three series of 24 syllables each.

I	II	III	IV	V	VI
122	73	45	29	21	16
127	73	40	25	18	15
154	78	47	27	18	12
139	61	33	17	12	10
133	73	36	26	18	14
142	66	42	26	17	14
124	70	36	24	16	14
m 134	71	40	25	17	14
P.E.$_m$ 2.9	1.4	1.3	1	0.7	0.5

3. Two series of 36 syllables each.

I	II	III	IV	V	VI
115	52	23	18	9	8
124	59	33	21	12	10
137	55	26	17	12	8
109	48	21	16	10	10
87	39	21	15	13	8
105	40	22	17	12	10
110	41	21	16	10	11
m 112	48	24	17	11	9
P.E.$_m$ 4	2	1.1	0.5	0.4	0.3

4. Six stanzas of Byron's "Don Juan" (Canto X).

I	II	III	IV
53	29	18	11
56	29	16	10
53	30	15	10
49	25	14	9
53	27	16	10
53	34	21	9
50	28	17	10
m 52	29	17	10
P.E.$_m$ 0.6	0.7	0.6	0.2

In order to bring out more clearly the separate relations which exist between the resulting averages, it is necessary to reduce the total figures to the same unit—*i.e.*, to divide them in each case by the number of series constituting a single trial. If this is done and the repetition necessary for the recital is deducted, the following table results, fractions being given to the nearest half or quarter.

Number of syllables in one series	Number of repetitions which, on the average, were necessary for the bare learning of the series on successive days					
	I	II	III	IV	V	VI
12	16.5	11.	7.5	5.	3.	2.5
24	44.	22.5	12.5	7.5	4.5	3.5
36	55.	23.	11.	7.5	4.5	3.5
1 stanza D. J.	7.75	3.75	1.75	0.5	(0)	(0).

From several points of view these numbers require further consideration.

Section 32. Influence of the Length of the Series

If the results for the first and second days are examined, welcome, though not surprising, supplementary data on the relation of dependence presented in Chapter V is obtained. In the former chapter, it was shown that, as the length of the series increased, the number of repetitions requisite increased very rapidly. Here, the result is that the effect of this need of more numerous repetitions in the cases investigated consists not merely in making the series just reproducible, but also in the firmer establishment of the longer series. After an interval of 24 hours they could be relearned to the point of being just reproducible with a saving both absolutely and relatively greater than with the shorter series.

The following table makes this relation clear.

Number of syllables in one series	Number of repetitions for learning	Saving in repetitions in relearning after 24 hours	Saving in % of requirement for first learning
12	16.5	5.5	33.3
24	44	21.5	48.9
36	55	32	58.2

The saving in the case of the shortest of the series investigated is one third for the second learning as compared with the first; while with the longest series, it is six tenths. It can be said, therefore, that by being learned to the first possible reproduction the series of 36 syllables is approximately twice as firmly established as the series of 12 syllables.

In this there is nothing new. On the basis of the familiar experience that that which is learned with difficulty is better retained, it would have been safe to prophesy such an effect from the greater number of repetitions. That which probably would not have been anticipated and which also demands attention, is the more definite determination of this general relation. So far as the numbers go, they seem to show that, between the increase of the repetitions necessary for the first learning and the inner stability of the series effected by them, there is no proportionality. Neither the absolute nor the relative saving of work advances in the same way as the number of repetitions;

the former advance much faster and the latter noticeably more slowly. It cannot, therefore, be said in any exact sense of the words that the more frequently a series needs to be repeated to-day in order to be learned by heart the more repetitions will be saved in its reproduction after 24 hours. The relation in force seems to be much more complicated and its exact determination would require more extensive investigations.

The relation of repetitions for learning and for repeating English stanzas needs no amplification. These were learned by heart on the first day with less than half of the repetitions necessary for the shortest of the syllable series. They acquired thereby so great stability that for their reproduction on the next day proportionally no more work was needed than for the series of 24 syllables—*i.e.*, about half of the first expenditure.

Section 33. Influence of Repeated Learning

We will now take into consideration the results for the successive days taken as a whole. On each day the average number of repetitions necessary for the committing of a given series is less than on the preceding day. With the longer series, in whose case the first output of energy is great, the decrease in the amount of work each time necessary to reach the first possible reproduction is proportionally rapid. With the shorter series, where the first output is small, the decrease is proportionally slow. On this account the numbers of repetitions necessary for the different series approach each other more and more. With the series of 24 and 36 syllables this is apparent even from the second day; from the fourth day on, the numbers fall absolutely together. And by the fifth day they have approached very closely to the number of repetitions still necessary, in accordance with the slower decrease, for the learning of the 12-syllable series.

A simple conformity to law cannot be discovered in this successively decreasing necessity for work. The quotients of the necessary repetitions on two successive days approach unity. If the final repetition were not subtracted, as was done in the concluding table of Section 31, but were reckoned in, this approach would be somewhat faster. (In the case of the English stanzas it generally takes place only under these conditions.)

Nevertheless the course of the numbers cannot be described by a simple formula.

Rather is this the case if one takes into consideration, not the gradually decreasing necessity for work, but the just as gradually decreasing saving of work.

No.	Number of syllables in one series	Number of repetitions saved on learning a series on the following day; average values				
		I-II	II-III	III-IV	IV-V	V-VI
1	12	5.5	3.5	2.5	2	0.5
2	24	21.5	10.0	5.0	3	1.0
3	36	32.0	12.0	3.5	3	1.0
4	1 stanza D. J.	4.0	2.0	1.25	0.5	...

Of these numerical sequences two—namely, the second and fourth rows—form with great approximation a decreasing geometrical progression with the exponent 0.5. Very slight changes in the numbers would be sufficient fully to bring out this conformity. By slight changes, the first row might also be transformed into a geometrical progression with the exponent 0.6. On the contrary, a large error in the results of investigation would need to be assumed in order to get out of Row 3 any such geometric progression (whose exponent would then be about one third).

If not for all, yet for most, of the results found, the relation can be formulated as follows: If series of nonsense syllables or verses of a poem are on several successive days each time learned by heart to the point of the first possible reproduction, the successive differences in the repetitions necessary for this form approximately a decreasing geometrical progression. In the case of syllable-series of different lengths, the exponents of these progressions were smaller for the longer series and larger for the shorter ones.

Although the tests just described were individually not more protracted than the others, yet relatively they required many days, and the average values were consequently derived from a rather small number of observations. So here, even more than elsewhere, I am unable to affirm that the simple conformity to law approximately realised in the results so far obtained would

stand the test of repetition or wider extension of research. I content myself by calling attention to it without emphasis.

Section 34. Influence of the Separate Repetitions

The problem of the present chapter is, as has already been pointed out, closely related to that of Chapter VI. In both cases the investigation concerns the influence of an increasing number of repetitions on the fixation of the series of syllables, a fixation made increasingly stronger thereby. In the former case the total number of repetitions immediately succeeded each other without regard to whether the spontaneous reproduction of the series was obtained through them or to how it was obtained. In this case the repetitions were distributed over several days and the attainment of the first possible reproduction was employed for their apportionment on the separate days. If, now, the results obtained in both cases have, at least for my own personality, any wider validity, we should expect that in so far as they are comparable, they would harmonise. That is, we should expect in this case as in the former that the effect of the later repetitions (therefore, those of the 2nd, 3rd, and later days), would at first be approximately as great as that of the earlier, and later would decrease more and more.

A more exact comparison is in the nature of the case not now possible. In the first place, the series of Chapter VI and the present ones are of different length. In the second place, the detailed ascertainment of the effect of the repetitions of the succssive days taken solely by themselves would be possible only through assumptions which might be plausible enough on the basis of the data presented, but which would be easily controvertible on account of the insecurity of these data.

We found, for example, that nine 12-syllable series were learned on six successive days by means of 158, 109, 75, 56, 37 and 31 repetitions. The effect of the first 158 repetitions is here immediately given in the 109 repetitions of the succeeding day in the difference, 158 — 109. But if we wish to know the intrinsic effect of these 109 repetitions, namely the saving effected by them, on the third day, we could not simply take the difference, 109 — 75. We should need to know, rather, with how many repetitions (x) the series would have been learned

on the third day if no repetitions had occurred on the second, and we should then have in the difference, $x - 75$, the separate effect of the 109 repetitions actually given. Since the forgetting increased somewhat from the second to the third day, x would be somewhat greater than 109. In the same way, for the determination of the effect of the 75 repetitions of the third day, we should need to learn in some way or other with how many repetitions (y), the series would have been learned by heart on the fourth day which, on the first day, required 158; and on the second, 109. The difference, $y - 56$, would then give the measure of that effect; and so on. For the ascertainment of x, the results of Chapter VII give a certain basis. There the result was that, in the case of 13-syllable series, the amount forgotten at the end of 24 hours was to that forgotten at the end of 2 x 24 hours as 66 to 72. But the employment of this relation, itself insecure, would be justifiable only in case of the 12-syllable series, and would accordingly not help in the determination of y, etc. One could at the best suppose that the resulting quotients would approximate yet more closely to unity.

Accordingly I renounce these uncertain assumptions, and content myself with presenting the relations of the successive repetitions to the successive savings by showing that the presupposed pure effect of the separate repetitions would be represented by somewhat greater and presumably less divergent numbers.

Number of syllables of each series	The following savings after 24 hours resulted from each repetition on the separate days (in fractions of their own value)				
	I	II	III	IV	V
12	0.31	0.31	0.25	0.34	0.16
24	0.47	0.44	0.38	0.32	0.16
36	0.57	0.50	0.29	0.35	0.18

Although the course of these figures, which are, as has been said, inexact as to their absolute values, is tolerably regular in the case of the 24-syllable series only, its general character agrees very well with what would be expected from the results of Chapter IV. The effect of the repetitions is at first approxi-

mately constant, the saving in work which results from these repetitions increases accordingly for a while proportional to their number. Gradually the effect becomes less; and finally, when the series has become so firmly fixed that it can be repeated almost spontaneously after 24 hours, the effect is shown to be decidedly less. The results of the fourth and the present chapter, as far as can be seen, support each other.

Nevertheless, there is a noteworthy distinction to which I call attention. We found above (p. 60) that six 12-syllable series, which had been learned at a given time with an average of 410 repetitions, could be learned by heart at the end of 24 hours with, on the average, 41 repetitions. For a single 12-syllable series, accordingly, 68 immediately successive repetitions had the effect of making possible an errorless recital on the following day after 7 repetitions. In the present research with distribution of the repetitions over several days the same effect appears on the fourth day: 9 12-syllable series were learned by heart with 56 repetitions. Each series, therefore, was learned with about 6 repetitions. But the number of repetitions which were necessary for the production of this effect in the case of the nine series amounted to only $158 + 109 + 75 = 342$. For a single series, therefore, the number was 38. For the relearning of a 12-syllable series at a definite time, accordingly, 38 repetitions, distributed in a certain way over the three preceding days, had just as favorable an effect as 68 repetitions made on the day just previous. Even if one makes very great concessions to the uncertainty of numbers based on so few researches, the difference is large enough to be significant. It makes the assumption probable that *with any considerable number of repetitions* a suitable distribution of them over a space of time is decidedly more advantageous than the massing of them at a single time.

With this result, found here for only very limited conditions, the method naturally employed in practice agrees. The schoolboy doesn't force himself to learn his vocabularies and rules altogether at night, but knows that he must impress them again in the morning. A teacher distributes his class lesson not indifferently over the period at his disposal but reserves in advance a part of it for one or more reviews.

CHAPTER IX

RETENTION AS A FUNCTION OF THE ORDER OF SUCCESSION OF THE MEMBERS OF THE SERIES

> "How odd are the connections
> Of human thoughts which jostle in their flight."

Section 35. Association according to Temporal Sequence and its Explanation

I shall now discuss a group of investigations made for the purpose of finding out the conditions of association. The results of these investigations are, it seems to me, theoretically of especial interest.

The non-voluntary re-emergence of mental images out of the darkness of memory into the light of consciousness takes place, as has already been mentioned, not at random and accidentally, but in certain regular forms in accordance with the so-called laws of association. General knowledge concerning these laws is as old as psychology itself, but on the other hand a more precise formulation of them has remained—characteristically enough—a matter of dispute up to the very present. Every new presentation starts out with a reinterpretation of the contents of a few lines from Aristotle, and according to the condition of our knowledge it is necessary so to do.

Of these "Laws," now—if, in accordance with usage and it is to be hoped in anticipation of the future, the use of so lofty a term is permitted in connection with formulae of so vague a character—of these laws, I say, there is one which has never been disputed or doubted. It is usually formulated as follows: Ideas which have been developed simultaneously or in immediate succession in the same mind mutually reproduce each other, and do this with greater ease in the direction of the original succession and with a certainty proportional to the frequency with which they were together.

This form of non-voluntary reproduction is one of the best verified and most abundantly established facts in the whole realm of mental events. It permeates inseparably every form of reproduction, even the so-called voluntary form. The function of the conscious will, for example, in all the numerous reproductions of the syllable-series which we have come to know, is limited to the general purpose of reproduction and to laying hold of the first member of the series. The remaining members follow automatically, so to speak, and thereby fulfill the law that things which have occurred together in a given series are reproduced in the same order.

However, the mere recognition of these evident facts has naturally not been satisfying and the attempt has been made to penetrate into the inner mechanism of which they are the result. If for a moment we try to follow up this speculation concerning the *Why*, before we have gone more than two steps we are lost in obscurities and bump up against the limits of our knowledge of the *How*.

It is customary to appeal for the explanation of this form of association to the nature of the soul. Mental events, it is said, are not passive happenings but the acts of a subject. What is more natural than that this unitary being should bind together in a definite way the contents of his acts, themselves also unified? Whatever is experienced simultaneously or in immediate succession is conceived in one act of consciousness and by that very means its elements are united and the union is naturally stronger in proportion to the number of times they are entwined by this bond of conscious unity. Whenever, now, by any chance one part only of such a related complex is revived, what else can it do than to attract to itself the remaining parts?

But this conception does not explain as much as it was intended to do. For the remaining parts of the complex are not merely drawn forth but they respond to the pull in an altogether definite direction. If the partial contents are united simply by the fact of their membership in a single conscious act and accordingly all in a similar fashion, how does it come about that a sequence of partial contents returns in precisely the same order and not in any chance combination? In order to make this intelligible, one can proceed in two ways.

In the first place it can be said that the connection of the things present simultaneously in a single conscious act is made from each member to its immediate successor but not to members further distant. This connection is in some way inhibited by the presence of intermediate members, but not by the interposition of pauses, provided that the beginning and end of the pause can be grasped in one act of consciousness. Thereby return is made to the facts, but the advantage which the whole plausible appeal to the unitary act of consciousness offered is silently abandoned. For, however much contention there may be over the number of ideas which a single conscious act may comprehend, it is quite certain that, if not always, at least in most cases, we include more than two members of a series in any one conscious act. If use is made of one feature of the explanation, the characteristic of unity, as a welcome factor, the other side, the manifoldness of the members, must be reckoned with, and the right of representation must not be denied it on assumed but unstatable grounds. Otherwise, we have only said,—and it is possible that we will have to be content with that—that it is so because there are reasons for its being so.

There is, consequently, the temptation to use this second form of statement. The ideas which are conceived in one act of consciousness are, it is true, all bound together, but not in the same way. The strength of the union is, rather, a decreasing function of the time or of the number of intervening members. It is therefore smaller in proportion as the interval which separates the individual members is greater. Let *a, b, c, d* be a series which has been presented in a single conscious act, then the connection of *a* with *b* is stronger than that of *a* with the later *c;* and the latter again is stronger than that with *d*. If *a* is in any way reproduced, it brings with it *b* and *c* and *d*, but *b*, which is bound to it more closely, must arise more easily and quickly than *c*, which is closely bound to *b*, etc. The series must therefore reappear in consciousness in its original form although all the members of it are connected with each other.

Such a view as this has been logically worked out by Herbart. He sees the basis of the connection of immediately successive ideas not directly in the unity of the conscious act, but in something similar: opposed ideas which are forced together in a

unitary mind can be connected only by partial mutual inhibition followed by fusion of what remains. Yet this, for our purpose, is not essential. He proceeds as follows:

"Let a series, *a, b, c, d* . . . be given in perception, then *a*, from the very first moment of the perception and during its continuance, is subjected to inhibition by other ideas present in consciousness. While *a*, already partially withdrawn from full consciousness, is more and more inhibited, *b* comes up. The latter, at first uninhibited, fuses with the retiring *a*. *c* follows and, itself uninhibited, is united with the fast dimming *b* and the still more obscured *a*. In a similar fashion *d* follows and unites itself in varying degrees with *a*, *b*, and *c*. Thus there originates for each of these ideas a law according to which, after the whole series has been forced out of consciousness for some time, in its own way on its renewed appearance each idea struggles to call up every other idea of the same series. Suppose that *a* arises first, it is more closely connected with *b*, less with *c*, still less with *d*, etc. But, taken in the reverse order *b*, *c*, and *d*, all in an uninhibited condition, are fused with what remains of *a*. Consequently *a* seeks to bring them completely back to the form of an uninhibited idea; but its effect is quickest and strongest upon *b*, slower on *c* and still slower on *d*, etc. (whereby closer inspection shows that *b* sinks again while *c* is still rising, and that in the same way *c* sinks while *d* rises, etc.). In short, the series runs off as it was originally given. If we suppose, on the contrary, that *c* was the one initially reproduced, then its effect on *d* and the succeeding members is similar to that revealed by *a*—*i.e.*, the series *c, d,* . . . runs off gradually in conformity with its order. *b* and *a*, however, experience an altogether different influence. With their separate conscious residues, the uninhibited *c* had fused; its effect upon *a* and *b* was therefore without loss of power and without delay, but this effect was limited to bringing back the conscious residues of *a* and *b* bound up with it, only a part of *b* and a still smaller part of *a* being recalled to consciousness. This, then, is what happens if the process of recall begins anywhere at the middle of a known series. That which preceded the point of recall rises at once in graded degrees of clearness. That which followed, on the contrary, runs off in the order of the original series. The series,

however, never runs backwards, an anagram is never formed out of a well understood word without voluntary effort."[1]

According to this conception, therefore, the associative threads, which hold together a remembered series, are spun not merely between each member and its immediate successor, but beyond intervening members to every member which stands to it in any close temporal relation. The strength of the threads varies with the distance of the members, but even the weaker of them must be considered as relatively of considerable significance.

The acceptance or rejection of this conception is clearly of great importance for our view of the inner connection of mental events, of the richness and complexity of their groupings and organisation. But it is clearly quite idle to contend about the matter if observation is limited to conscious mental life, to the registration of that which whirls around by chance on the surface of the sea of life.

For, according to the hypothesis, the threads which connect one member to its immediate successor although not the only one spun, are, however, stronger than the others. Consequently, they are, in general, as far as appearances in consciousness are concerned, the important ones, and so the only ones to be observed.

On the other hand, the methods which lie at the basis of the researches already described permit the discovery of connections of even less strength. This is done by artificially strengthening these connections until they reach a definite and uniform level of reproducibility. I have, therefore, carried on according to this method a rather large number of researches to test experimentally

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[1] Herbart, Lehrb. z. Psychol., Sect. 29. A similar "pleasing" view, as he calls it, was developed by Lotze, Metaphysik (1879) p. 527, with the modification that he attempts to eliminate the notion of varying strength of the ideas, which view he rejects. In accordance with the view mentioned first above, he sees the real reason for a faithful reproduction of a series of ideas in the fact that association is made only from one link to the following link. Accordingly, he teaches, in his Lectures on Psychology (p. 22), "Any two ideas, regardless of content, are associated when they are produced either simultaneously or in immediate succession—*i.e.*, without an intervening link. And upon this can be based without further artifice the special ease with which we reproduce a series of ideas in their proper order but not out of that order. By "further artifice" he seems to mean Herbart's attempt at an arrangement.

in the field of the syllable-series the question at issue, and to trace an eventual dependence of the strength of the association upon the sequence of the members of the series appearing in succession in consciousness.

Section 36. Methods of Investigation of Actual Behavior

Researches were again carried out with six series of 16-syllables each. For greater clearness the series are designated with Roman numbers and the separate syllables with Arabic. A syllable group of the following form constituted, then, each time the material for research:

I(1) I(2) I(3) .... I(15) I(16)
II(1) II(2) II(3) .... II(15) II(16)
.
.
VI(1) .... VI(15) VI(16)

If I learn such a group, each series by itself, so that it can be repeated without error, and 24 hours later repeat it in the same sequence and to the same point of mastery, then the latter repetition is possible in about two thirds of the time necessary for the first.[1] The resulting saving in work of one third clearly measures the strength of the association formed during the first learning between one member and its immediate successor.

Let us suppose now that the series are not repeated in precisely the same order in which they were learned. The syllables learned in the order I(1) I(2) I(3) . . . I(15) I(16) may for example be repeated in the order I(1) I(3) I(5)

[1] I have omitted to present a few tests with series of 16 syllables each from which this number was obtained, because the results of the sixth chapter sufficiently cover this point. There (p. 55), we saw that six series of 16 syllables each, each series being repeated 32 times, could be memorised after 24 hours in an average of 863 seconds. 32 repetitions are, on an average, just necessary to bring about the first possible reproduction of series of 16 syllables each. Considering the close proportion which exists between the number of repetitions on a given day and the saving of work on the next, it cannot much matter whether the series were repeated, each 32 times, or were memorised each to the first possible reproduction. Since the latter requires about 1,270 seconds, the work of repetition on the following day amounts, as stated above, to about two thirds of this time. The relative saving when 16-syllable series are relearned after 24 hours, is, therefore, scarcely different from that found for series of 12 and 13 syllables (Chapters VII and VIII), while it gradually increases for still greater length of series.

. . . I(15) I(2) I(4) I(6) . . . I(16), and the remaining series with a similar transformation. There will first be, accordingly, a set composed of all the syllables originally in the odd places and then a set of those originally in the even places, the second set immediately following the first. The new 16-syllable series, thus resulting, is then learned by heart. What will happen? Every member of the transformed series was, in the original series, separated from its present immediate neighbor by an intervening member with the exception of the middle term where there is a break. If these intervening members are actual obstructions to the associative connection, then the transformed series are as good as entirely unknown. In spite of the former learning of the series in the original sequence, no saving in work should be expected in the repetition of the transformed series. If on the other hand in the first learning threads of association are spun not merely from each member to its immediate successor but also over intervening members to more distant syllables, there would exist, already formed, certain predispositions for the new series. The syllables now in succession have already been bound together secretly with threads of a certain strength. In the learning of such a series it will be revealed that noticeably less work is required than for the learning of an altogether new series. The work, however, will be greater than in relearning a given series in unchanged order. In this case, again, the saving in work will constitute a measure of the strength of the associations existing between two members separated by a third. If from the original arrangement of the syllables new series are formed by the omission of 2, 3, or more intervening members, analogous considerations result. The derived series will either be learned without any noticeable saving of work, or a certain saving of work will result, and this will be proportionally less as the number of intervening terms increases.

On the basis of these considerations I undertook the following experiment. I constructed six series of 16 syllables each with the latter arranged by chance. Out of each group a new one was then constructed also composed of six series of 16 syllables each. These new groups were so formed that their adjacent syllables had been separated in the original series by either 1, or 2, or 3, or 7 intervening syllables.

If the separate syllables are designated by the positions which they held in the original arrangement, the following scheme results:

```
I(1)  I(2)  I(3)............................................ I(15)  I(16)
II(1) II(2) II(3)........................................... II(15) II(16)
 .                                                                    .
 .                                                                    .
VI(1)..............................................................VI(16)
```

By using the same scheme the derived groups appear as follows:

By Skipping 1 Syllable

```
 I(1)  I(3)  I(5)................ I(15)  I(2)  I(4)  I(6)................ I(16)
II(1) II(3) II(5)................II(15) II(2) II(4) II(6)................II(16)
 .                                                                          .
 .                                                                          .
VI(1) VI(3)......................VI(15) VI(2) VI(4).......................VI(16)
```

By Skipping 2 Syllables

```
 I(1)  I(4)  I(7) I(10) I(13)  I(16)  I(2)  I(5)  I(8) I(11)  I(14)  I(3)  I(6)  I(9) I(12)  I(15)
II(1) II(4) II(7)............ II(16) II(2) II(5)............ II(14) II(3) II(6)............ II(15)
 .                                          .                                                 .
 .                                          .                                                 .
VI(1) VI(4)..................VI(16)  VI(2) VI(5)............VI(14) VI(3) VI(6)........... VI(15)
```

By Skipping 3 Syllables

```
 I(1)  I(5) I(9) I(13)  I(2)  I(6) I(10) I(14)  I(3)  I(7) I(11) I(15)  I(4)  I(8) I(12)  I(16)
II(1) II(5)............ II(2) II(6)............ II(3) II(7)............ II(4) II(8)...... II(16)
 .                                    .                                                     .
 .                                    .                                                     .
VI(1) VI(5)............ VI(2) VI(6)............ VI(3) VI(7)............ VI(4) VI(8)......VI(16)
```

By Skipping 7 Syllables

```
  I(1)    I(9)  II(1)   II(9) III(1) III(9) IV(1)  IV(9)  V(1)  V(9)  VI(1)  VI(9)   I(2)   I(10)  II(2)  II(10)
III(2) III(10) IV(2)  IV(10)   V(2)  V(10) VI(2) VI(10)  I(3) I(11)  II(3) II(11) III(3) III(11) IV(3)  IV(11)
  .                                            .                                                       .
  .                                            .                                                       .
  V(7)   V(15) VI(7)  VI(15)   I(8)  I(16) II(8) II(16) III(8) III(16) IV(8) IV(16)  V(8)  V(16)  VI(8)  VI(16)
```

As a glance at this scheme will show, not all the neighboring syllables of the derived series were originally separated by the number of syllables designated. In some places in order to again obtain series of 16 syllables, greater jumps were made; but in no case was the interval less. Such places are, for example, in the series in which two syllables are skipped, the transitions from I(16) to I(2) and from I(14) to I(3). In the series in which 7 intermediates were jumped, there are seven places where there was no previous connection between successive syllables since the syllables in question came from different series and the different series, as has been often mentioned, were learned independently. The following is given in illustration: I(9) II(1), II(9) III(1), etc. The number of these breaks varies with the different kinds of derivation, but in each case is the same as the number of skipped syllables. On account of this difference,

the derived series suffer from an inequality inherent in the nature of the experiment.

In the course of the experiment the skipping of more than 7 syllables was shown to be desirable, but I refrained from carrying that out. The investigations with the six 16-syllable series were carried quite far; and if series had been constructed using greater intervals, the breaks above mentioned would have had too much dominance. The derived series then contained ever fewer syllable-sequences for which an association was possible on the basis of the learning of the original arrangement; they were ever thus more incomparable.

The investigations were carried on as follows:—Each time the six series were learned in the original order and then 24 hours later in the derived and the times required were compared. On account of the limitation of the series to those described above the results are, under certain circumstances, open to a serious objection. Let it be supposed that the result is that the derived series are actually learned with a certain saving of time, then this saving is not necessarily due to the supposed cause, an association between syllables not immediately adjacent. The argument might, rather, run as follows. The syllables which are first learned in one order and after 24 hours in another are in both cases the same syllables. By means of the first learning they are impressed not merely in their definite order but also purely as individual syllables; with repetition they become to some extent familiar, at least more familiar than other syllables, which had not been learned just before. Moreover the new series have in part the same initial and final members as the old. Therefore, if they are learned in somewhat less time than the first series required, it is not to be wondered at. The basis of this does not necessarily lie in the artificial and systematic change of the arrangement, but it possibly rests merely on the identity of the syllables. If these were repeated on the second day in a new arrangement made entirely by chance they would probably show equally a saving in work.

In consideration of this objection and for the control of the remaining results I have introduced a further, the fifth, kind of derived series. The initial and final syllables of the original series were left in their places. The remaining 84 syllables, intermediates, were shaken up together and then, after chance

drawing, were employed in the construction of new series between the original initial and final series. As a result of the learning of the original and derived series there must in this case also be revealed how much of the saving in work is to be ascribed merely to the identity of the syllable masses and to the identity of the initial and final members of the separate series.

Section 37. Results. Associations of Indirect Sequence

For each group of original and derived series 11 double tests were instituted, 55 therefore in all. These were distributed irregularly over about 9 months. The results were as follows:

1) With derivation of the series by skipping one intermediate syllable.

The original series were learned in x seconds	The corresponding derived series, in y seconds	The latter, therefore, with a saving of z seconds
x=	y=	z=
1187	1095	92
1220	1142	78
1139	1107	32
1428	1123	305
1279	1155	124
1245	1086	159
1390	1013	377
1254	1191	63
1335	1128	207
1266	1152	114
1259	1141	118
m 1273	1121	152

2) With derivation of the series by skipping two intermediate syllables.

x=	y=	z=
1400	1185	215
1213	1252	—39
1323	1245	78
1366	1103	263
1216	1066	150
1062	1003	59
1163	1161	2
1251	1204	47
1182	1086	96
1300	1076	224
1276	1339	—63
m 1250	1156	94

3) **With derivation of the series by skipping three intermediate syllables.**

The original series were learned in x seconds	The corresponding derived series, in y seconds	The latter, therefore, with a saving of z seconds
x=	y=	z=
1282	1347	—65
1202	1131	71
1205	1157	48
1303	1271	32
1132	1098	34
1365	1235	130
1210	1145	65
1364	1176	188
1308	1175	133
1298	1209	89
1286	1148	138
m 1269	1190	78

4) **With derivation of the series by skipping seven intermediate syllables.**

x=	y=	z=
1165	1086	79
1265	1295	—30
1197	1091	106
1295	1254	41
1233	1207	26
1335	1288	47
1321	1278	43
1344	1275	69
1322	1328	—6
1224	1212	12
1294	1217	77
m 1272	1230	42

5) **With derivation of the series by retaining the beginning and end syllables and arranging the remainder by chance.**

1305	1302	3
1181	1259	—78
1207	1237	—30
1401	1277	124
1278	1271	7
1302	1301	1
1248	1379	—131
1237	1240	—3
1355	1236	119
1214	1142	72
1147	1101	46
m 1261	1250	12

To summarize the results: The new series formed by skipping 1, 2, 3 and 7 intermediate members were learned with an average saving of 152, 94, 78 and 42 seconds. In the case of the construction of a new series through a mere permutation of the syllables, there was an average saving of 12 seconds.

In order to determine the significance of these figures, it is necessary to compare them with the saving in work in my case in the relearning of an unchanged series after 24 hours. This amounted to about one third of the time necessary for the first learning in the case of 16-syllable series, therefore about 420 seconds.

This number measures the strength of the connection existing between each member and its immediate sequent, therefore the maximal effect of association under the conditions established. If this is taken as unity, then the strength of the connection of each member with the second following is a generous third and with the third following is a scant fourth.

The nature of the results obtained confirm—for myself and the cases investigated—the second conception given above and explained by means of a quotation from Herbart. With repetition of the syllable series not only are the individual terms associated with their immediate sequents but connections are also established between each term and several of those which follow it beyond intervening members. To state it briefly, there seems to be an association not merely in direct but also in indirect succession. The strength of these connections decreases with the number of the intervening numbers; with a small number it was, as will be admitted, of surprising and unanticipated magnitude.

No evidence has been secured, however, establishing the facilitation of the process of relearning a series by means of the identity of the syllables and the identity of the initial and final terms.

Section 38. Experiments with Exclusion of Knowledge

I have hitherto not stated the probable errors of the results in order to discuss their reliability more fully at this time.

When I started upon the experiment I had no decided opinion in favor of the final results. I did not find facilitation of the learning of the derived series essentially more plausible than the opposite. As the numbers more and more bespoke the existence

of such facilitation, it dawned upon me that this was the correct and natural thing. After what has been said above (p. 27ff) one might think that in the case of the remaining experiments, this idea has possibly favored a more attentive and therefore quicker learning of the derived series, and so has, at least, decidedly strengthened the resulting saving in work, even if it has not caused it altogether.

For the three largest of the numbers found,—consequently, for the facilitation of the work which took place in the case of the omission of 1, 2, and 3 intervening syllables—this objection is of slight significance. For these are proportionately so large that it would be attributing too much to an involuntary heightening of a state of attention, voluntarily concentrated without this to the utmost, if an actual influence is ascribed to it here. Moreover, the gradation of the numbers, decisively issuing as they do from the distribution of the individual values and running parallel with the number of skipped intermediate terms, is inconceivable on any such hypothesis as this. For the supposed greater concentration of the attention could clearly work only in general. How could it possibly bring about so regular a gradation of numbers in the case of tests which were separated from each other by weeks and months?

The objection presented above could render doubtful only the fourth result, the proportionally slight saving in the learning of series formed from other series by skipping seven intermediate terms.

Clearly in this case the exact determination of the difference is of especial interest because of the significant size of the interval over which an association took place.

In the case of the present investigations there exists the possibility of so arranging them that knowledge concerning the outcome of the gradually accumulating results is excluded and so that consequently the disturbing influence of secret views and desires disappears. I have accordingly instituted a further group of 30 double tests in the following way as a control of the above results, and especially of the least certain of them.

On the front side of a page were written six 16-syllable series selected by chance and on the reverse side of the same sheet six series formed from them by one of the methods of derivation described above (p. 97). For each of the five transforma-

tions 6 sheets were prepared. The fronts and backs of these could be easily distinguished but not the sheets themselves. The thirty sheets were shuffled together and then laid aside until any memory as to the occurrence of the separate syllables in definite transformations could be considered as effaced. Then the front side, and 24 hours later, the reverse side of a given sheet were learned by heart. The times necessary for learning the separate series were noted but they were not assembled and further elaborated until all [illegible] sheets had been completed. Following are the numbers.

1) With derivation of series transformed by skipping one intermediate syllable.

The original series were learned in x seconds	The corresponding derived series, in y seconds	The latter, therefore, with a saving of z seconds
x=	y=	z=
1137	1081	56
1292	1045	247
1202	1237	—35
1272	1202	70
1436	1299	137
1340	1157	183
m 1280	1170	110

2) With derivation of the series by skipping two intermediate syllables.

x=	y=	z=
1415	1232	183
1201	1290	—89
1291	1156	135
1358	1153	205
1232	1254	—22
1168	1107	61
m 1278	1199	79

3) With derivation of the series by skipping three intermediate syllables

x=	y=	z=
1205	1166	39
1339	1068	271
1179	1293	—114
1238	1196	42
1257	1231	26
1240	1122	118
m 1243	1179	64

4) With derivation of the series by skipping seven intermediate syllables.

The original series were learned in x seconds	The corresponding derived series, in y seconds	The latter, therefore, with a saving of z seconds
x=	y=	z=
1191	1120	71
1191	1185	6
1237	1295	—58
1350	1306	44
1308	1260	48
1289	1158	131
m 1261	1221	40

5) With derivation of the series by retaining the first and last syllables in position and changing the rest by chance.

x=	y=	z=
1305	1180	125
1206	1205	1
1310	1426	—116
1163	1089	74
1272	1388	—116
1309	1305	4
m 1261	1266	—5

By derivation of the transformed series by skipping 1, 2, 3, 7 intermediate syllables, the derived series were therefore learned with an average saving of 110, 79, 64, 40 seconds. On the contrary with derivation of the series by permutation of the syllables the learning required an average *increase* in expenditure of 5 seconds.

Taken as a whole, these last results exactly confirm, as can be seen, the result that was obtained at the beginning. The number of these experiments was proportionally small and, during the course of each experiment, there was complete exclusion of knowledge as to results. In spite of these facts and although the numbers, considered individually, seem to be distributed without regard to law, their grouping, when taken as a whole, is seen to be in conformity to a simple law. The fewer are the intervening members which separate two syllables of a series which has been learned by heart, the less is the resistance offered by these separated syllables to their being learned in a new order.

And, in the same way, the fewer are these intervening terms, the stronger are the bonds which, as a result of the learning of the original series, connect the two syllables across the intervening members.

In addition to agreeing in their general course, the numbers for both groups of experiments also agree in the following respect. The difference between the first and second numbers has the greatest value, and that between the second and third has the least value. On the other hand, it is surprising that, with respect to their absolute size, the numbers of the second group are throughout smaller than those of the first. Two causes may be brought forward in explanation of this behavior, which, considering the conformity of the numbers, can scarcely be accidental. It may be that here is actually revealed that influence of expectation which has already been mentioned. On the basis of this hypothesis, the explanation of the fact that the numbers of the first group come out somewhat too large is that, in the course of the experiment, the existence of a saving in work in the case of the derived series was anticipated, and for this reason the learning of the series took place involuntarily with a somewhat greater concentration of attention. On the other hand, it may be that, in consequence of the excluded knowledge, there has been at work in the case of the numbers of the second group a disturbing element which has made them smaller. Here, to be sure, during the learning of the derived series a very lively curiosity developed concerning the category of transformation to which the series which had just been learned belonged. That this must have had a distracting, and therefore retarding, influence is probable not only in itself but also through the result obtained from the series derived by permutation of syllables. It was to be expected that the identity of the syllables, as well as of the initial and end terms, would make itself felt in this case by a saving of work, however small that saving might be. The latter effect appears, it is true, in the experiments of the first group. With those of the second group, however, there is noticeable, instead of this saving of work, a slight additional expenditure of time. This, if it is not merely accidental, can scarcely be explained otherwise than through the distracting curiosity mentioned.

It is possible that both influences were at work simultaneously

so that the first experiments gave results which were somewhat too high; and the second, results that were somewhat too low. It is allowable, under this hypothesis, to put the two sets of figures together so that the contrasting errors may compensate each other. In this way there was finally obtained out of the 85 double tests the following table.

Number of intermediate syllables skipped in the formation of the derived series	Time for learning the original series	Time for learning the derived series	Saving of work in learning the derived series	Probable error of saving of work*	Saving of work in % of original learning time
	(The numbers of the four middle columns denote seconds)				
0	(1266)	(844)	(422)		(33.3)
1	1275	1138	137	± 16	10.8
2	1260	1171	89	± 18	7.0
3	1260	1186	73	± 13	5.8
7	1268	1227	42	± 7	3.3
permutation of syllables	1261	1255	6	± 13	0.5

* The probable errors are calculated from the separate values for savings of work, while the latter, which were actually obtained by subtraction, are considered as the results of direct observation. (See p. 67, note.)

Section 39. Discussion of Results

In the foregoing table an especial interest, it seems to me, is connected with the last, and also with the next to the last, row of numbers. When there was complete identity of all the syllables and the initial and end terms were left in their places, the average saving of time for 17 tests dealing with the learning of the derived series was so slight that it was hardly to be determined. It fell within half of its probable error. The syllables were, therefore, in themselves, outside of their connection, so familiar to me that they did not become noticeably more familiar after being repeated 32 times. On the contrary when a related series was repeated the same number of times, each syllable became so firmly bound to the syllable which followed 8 places beyond that 24 hours later the influence of this connection could be determined in no doubtful fashion. It attains a value 6 times the probable error. Its existence, therefore, must be considered to be fully proved although naturally we cannot be so sure that its size is exactly what it was found

to be in the experiments. Although its absolute value is small, yet its influence amounts to one tenth of that of the connection which binds every member to its immediate successor. It is so significant, and at the same time the decrease in the after-effect of connections which were formed over 2, 3, 7 intervening members is so gradual a one, that the assertion can be made, on these grounds alone, that even the terms which stand still further from one another may have been bound to each other subconsciously by threads of noticeable strength at the time of the learning of the series.

I will summarise the results so far given in a theoretical generalisation. As a result of the repetition of the syllable-series certain connections are established between each member and all those that follow it. These connections are revealed by the fact that the syllable-pairs so bound together are recalled to mind more easily and with the overcoming of less friction than similar pairs which have not been previously united. The strength of the connection, and therefore the amount of work which is eventually saved, is a decreasing function of the time or of the number of the intervening members which separated the syllables in question from one another in the original series. It is a maximum for immediately successive members. The precise character of the function is unknown except that it decreases at first quickly and then gradually very slowly with the increasing distance of the terms.

If the abstract but familiar conceptions of 'power,' 'disposition,' be substituted for the concrete ideas of saving in work and easier reproduction, the matter can be stated as follows. As a result of the learning of a series each member has a tendency, a latent disposition, to draw after itself, at its own return to consciousness, all the members of the series which followed it. These tendencies are of varying strength. They are the strongest for the members which immediately follow. These tendencies are accordingly in general most easily demonstrable in consciousness. The series will return in its original form without the intervention of other influences while the forces directed to the resuscitation of the remaining members can be explicitly demonstrated only by the introduction of other conditions.

It is naturally not conceivable that by a mere caprice of nature the validity of the principles discovered should be limited exclusively to the character of the material in which they were obtained—*i.e.*, to series of nonsense syllables. They may be assumed to hold in an analogous way for every kind of idea-series and for the parts of any such series. It goes without saying, wherever relations exist between the separate ideas, other than those of temporal sequence and separation by intermediate members, these forces will control the associative flow, not exclusively, but with reference to all the modifications and complications introduced by relations of various affinities, connection, meaning, and the like.

At any rate, it will not be denied that the doctrine of Association would gain through a general validity of these results a genuine rounding out and, so to say, a greater reasonableness. The customary formulation, "ideas become associated if they are experienced simultaneously or in immediate succession," has something irrational about it. If the immediacy of succession is taken precisely, the principle contradicts the most common experiences. If it is not taken exactly, then it is hard to state what kind of sequence is properly meant. At the same time it is not clear why a sequence not quite direct should have an advantage which suddenly disappears in the case of a sequence still more indirect. As we now know, the directness or indirectness of the sequence is without effect upon the general nature of what happens between ideas which succeed each other. In both cases connections are formed which on account of their complete similarity can be designated only by the common term, Association. But these are of different strength. As the succession of united ideas approaches ideal immediacy the connecting threads grow stronger, and in proportion as it departs from this ideal, these threads grow weaker. The associations between more distant terms, although actually present and demonstrable under proper conditions have, nevertheless, on account of their slight strength, practically no significance. The associations between adjacent terms are, on the contrary, of relatively great importance, and will make their influence abundantly felt. Of course, if the series were left entirely to themselves and if they were always produced in precisely the same order, for each term there would appear only one association, the relatively

strongest—namely, that with the immediately succeeding term. But series of ideas are never left to themselves. The rich and quickly changing order of events brings them into the most manifold relations. They return with their members in the most varied combinations. And then, under certain circumstances, the stronger of these less strong associations between more distant terms must find opportunity to authenticate their existence and to enter into the inner course of events in an effective way. It is easy to see how they must favor a more rapid growth, a richer differentiation, and a many-sided ramification of the ideas which characterise the controlled mental life. Of course they also favor a greater manifoldness, and so apparently a greater arbitrariness and irregularity, in mental events.

Before I proceed further, I wish to add a few words concerning the above mentioned (*p.* 91) derivation of the association of successive ideas from the unitary consciousness of a unitary soul. There is a certain danger in bringing together a present result with one found previously. I mentioned above (p. 47) that the number of syllables which I can repeat without error after a single reading is about *seven*. One can, with a certain justification, look upon this number as a measure of the ideas of this sort which I can grasp in a single unitary conscious act. As we just now saw, associations are formed of noticeable strength over more than seven intervening members, therefore between the beginning and end of a nine-syllable series. And on account of the size of the numbers obtained and the nature of their gradation, it seems probable that, even with a larger number of syllables, connections would be formed between their extremes. If, however, associations are built between members too far separated to be held together in a single conscious act, it is no longer possible to explain the presence of those associations on the basis of the simultaneous presence of the united ideas in consciousness.

However, I recognise that those for whom such a derivation is a cherished matter are not necessarily forced by the above discussion to abandon their conception. Such are those who consider the unitary acts of a unitary soul as something more original, intelligible, transparent or better worthy of belief than the simple facts of association described above, so that the reduction of the latter to the former would be a noteworthy

achievement. One needs but to say that, in the case of an unfamiliar sequence of syllables, only about seven can be grasped in one act, but that with frequent repetition and gradually increasing familiarity with the series this capacity of consciousness may be increased. So, for example, a series of 16 syllables, which have been thoroughly memorised, may be present in a single conscious act. Accordingly this "explanation" is freely available. Those for whom it was of value in the case of association by simultaneity or immediate succession can employ it fully as well for our case of indirect sequence. And because of the modest requirements which in psychology are so often imposed upon explanations, this view will doubtless for a long time serve to make dim the vision and so prevent the frank recognition of this as one of the most wonderful of all riddles, and it will also act as a hindrance in the search for its true understanding.

Section 40. Reverse Associations

Of the many problems which spring out of the results presented I have been able for the time being to investigate only a few and these by means of only a small number of experiments.

As a result of the frequent repetition of a series—*a, b, c, d* . . . —certain connections—*ab, ac, ad, bd,* etc.—are formed. The idea *a,* whenever and however it returns to consciousness, has certain tendencies of different strength to bring also with it to consciousness the ideas *b, c, d.* Are now these connections and tendencies reciprocal? That is, if at any time *c* and not *a* is the idea by some chance revived, does this have, in addition to the tendency to bring *d* and *e* back with it, a similar tendency in the reverse direction towards *b* and *a?* In other words:—As a result of the previous learning of *a, b, c. d,* the sequences *a, b, c, a, c, e,* are more easily learned than any grouping of equal length of syllables previously unknown such as *p, q, r.* . . . Is the same thing true of the sequences *c b a,* and *e c a?* As a result of manifold repetition of a series are associations also formed in the reverse order?

The views of the psychologists seem to be divergent upon this point. One side call attention to the undoubted fact that in spite of complete mastery of, say, the Greek alphabet a person

is not at all in a position to repeat it readily backwards if he has not specially studied and practiced it in this form.

The other side make extensive use of reverse associations, as of something quite intelligible, in their explanation of the origin of voluntary and purposive movements. According to them the movements of the child are at first involuntary and accidental. With certain combinations of these, intensely pleasurable feelings result. In the case of movements as of feelings, memory traces remain which, by repetition of the occurrences, are always more closely associated with each other. If this connection has attained a certain strength, the mere idea of the agreeable feeling leads backwards to the idea of the movement which aroused it; then comes the actual movement and with it also the actual sensed feeling.

The conception of Herbart, which we learned to know above (p. 94), holds the middle course between these two views. The idea *c*, which appears in the course of a series, fuses with the ideas *b* and *a*, which have preceded it and which are yet present although becoming dim. If *c* is later on reproduced, it brings *b* and *a* with it but dimmed, not fully uninhibited or clearly conscious. With the sudden arousal of a member out of the midst of a series we survey that which preceded "at once in graded clearness"; but never does it happen that the series runs off in reverse order. To the member which springs up in consciousness there succeed in due order and in complete consciousness those terms which followed it in the original series.

For the purpose of testing the actual relations I carried out an experiment entirely similar to the previously described investigations. Out of groups each composed of six 16-syllable series arranged by chance new groups were derived either through mere reversal of the sequence or by that plus the skipping of an intermediate syllable. Then the two sets of groups were learned by heart, the derived form 24 hours later than the original.

If the scheme for the original form is written as follows:

I(1) I(2) I(3).......I(15) I(16), then the corresponding derived series is thus designated:

In the case of mere reversal of the syllable sequence;

I(16) I(15) I(14)........I(2) I(1),

In the case of reversal plus skipping of an intermediate syllable, I(16) I(14) I(12)....I(4) I(2) I(15) I(13)....I(3) I(1). For the first kind of derivation I have carried out ten experiments; for the second, only four.

The results are as follows:

1) With derivation of the transformed series by mere reversal of the syllable sequence.

The original series were learned in x seconds	The corresponding derived series, in y seconds	The latter, therefore, with a saving of z seconds
x=	y=	z=
1172	1023	149
1317	1170	147
1213	977	236
1202	1194	8
1257	1031	226
1210	1087	123
1285	1051	234
1260	1150	110
1245	1070	175
1329	1189	140
m 1249	1094	155 P.E.$_m$=15

In relation to the time of learning the original series the saving amounts to 12.4 per cent.

2) With derivation of the transformed series by reversal and at the same time by skipping one intermediate syllable.

x=	y=	z=
1337	1291	46
1255	1164	91
1158	1143	15
1313	1224	89
m 1266	1206	60 P.E.$_m$=12

In relation to the time of learning the original series the saving amounted to 5 per cent.

As a result of the learning of a series certain connections of the members are therefore actually formed in a reverse as well as in a forward direction. These connections are revealed in this way, that series which are formed out of members thus

connected are more easily learned than similar series, whose individual members are just as familiar but which have not been previously connected. The strength of the predispositions thus created was again a decreasing function of the distance of the members from each other in the original series. It was, however, considerably less for the reverse connections than for the forward ones, the distances being equal. With an approximately equal number of repetitions of the series the member immediately preceding a given member was not much more closely associated with it than the second one following it; the second preceding—so far as may be determined on the basis of these few researches—scarcely as firmly as the third following.

If one could assume a more general validity for this relation found here first in connection with syllable series, the mutually opposed experiences just mentioned would, I believe, become thoroughly intelligible. Where a series consists of only two members—as in the case of the connection between a simple idea of movement and that of an agreeable feeling—then, by means of frequent repetition the end term will acquire so strong a tendency to call up after itself the initial term that the latter will actually appear. For the bringing up of the term first preceding it is the only thing for which, as a result of the many repetitions, the second term has acquired a predisposition. But, no matter how many repetitions there may be in the case of a long series, it will never happen on the arousal of a middle term that the series will reappear in a reverse order. For, however easily the immediately preceding term may connect itself with the one for the moment aroused, the immediately succeeding term will appear more easily by far, and so will win the victory, provided other influences do not intervene.

No matter how thoroughly a person may have learned the Greek alphabet, he will never be in a condition to repeat it backwards without further training. But if he chances to set out purposely to learn it backwards, he will probably accomplish this in noticeably shorter time than was the case in the previous learning in the customary order. The objection is not in point that a poem or speech which has been committed to heart is not necessarily learned more quickly backwards than it was originally forwards. For with the learning in reverse direction the numerous threads of inner connection on which rapid learn-

ing of meaningful material in general depends will be brought to nothing.

Section 41. The Dependence of Associations of Indirect Sequence upon the Number of Repetitions

The connection set up as a result of many repetitions between the immediately succeeding members of an idea- or syllable-series is a function of the number of repetitions. As a result of the investigations of Chapter VI, which were purposely directed to the discovery of this relation, an approximate proportionality, within tolerably wide limits, has been made out between the number of repetitions and the strength of the connections established by them. The latter was measured, precisely as in the investigations of the present chapter, by the amount of work saved in relearning the connected series after 24 hours.

If now, as a result of repetitions, connections are also set up between members of a series which are not immediately successive, the strength of the latter is naturally also in some way dependent upon the number of repetitions. The question arises in what form the different dependence occurs in this case. Does a proportionality exist here also? If the number of repetitions is made greater, will the threads of separate strength, which bind together all the members of a series learned by heart, increase in strength in the same proportion? Or is the nature and rate of their increase in strength a different one as is the case with the strength of the threads themselves? On the basis of our present knowledge neither the one nor the other of these possibilities can be declared self-evident.

To facilitate an insight into the actual conditions I have instituted a few preliminary experiments in the following way. Six series of 16 syllables each were impressed upon the memory by a 16- or 64-fold attentive repetition. After 24 hours an equal number of derived series of the same length, which had been obtained from those already learned by skipping one intermediate syllable, were learned by heart to the first repetition. In order to make the investigations useful in other ways, the series were derived in this case by a method somewhat different from that described above (p. 97). The latter method differs

from the former in that here the odd-numbered syllables of the original series were not followed by the even-numbered syllables of the same series. But all the odd-numbered syllables of two original series were united to form a new 16-syllable series. Then the even-numbered syllables of the same original series were united to form a second new series. The scheme of the derived series was therefore not, as above,

I(1) I(3) I(5)................................ I(15) I(2) I(4)...... I(16).
II(1) II(3) II(5)................................ II(15) II(2) II(4)...... II(16),

but rather

I(1) I(3) I(5)................................ I(15) II(1) II(3)...... II(15)
I(2) I(4) I(6)................................ I(16) II(2) II(4)...... II(16)

The effect of the derivation upon the learning of the derived series, cannot, as it seems, be essentially affected through this slight change. Here, as in the above described method of derivation, the syllables which during the first learning had been separated from each other by an intervening syllable were learned 24 hours later in immediate succession.

For each number of repetitions used in learning I made 8 double tests, which gave the following results:

Number of repetitions employed for the impression of each of the original series:	16	64
Number of seconds required for learning the six derived series after 24 hours (including the recital):		
	1178	1157
	1216	982
	1216	1198
	950	1148
	1358	995
	1019	1017
	1191	1183
	1230	1196
Average	1170	1109
Probable error	30	22

On account of the small number of experiments the resulting averages are, unfortunately, not very exact; but the general character of the results would remain the same even if we considered the value false within the whole range of the probable error. This character becomes apparent upon comparison with the values given above (p. 56) for learning by heart six 16-syllable series which had not previously been learned. This took place in 1,270 seconds. After the original series had been

repeated 16 times, the derived series was learned with a saving of about 100 seconds; after repetition 64 times, with a like saving of 161 seconds. Quadrupling the repetitions resulted in increasing the saving only a little more than half as much again. The increase in strength of the associations reaching over an intermediate member was in nowise proportional to the number of repetitions, for the cases studied, not even within the limits for which this was noticeably the case for associations from one member to its immediate successor. On the contrary the effect of the repetitions in the case of associations of indirect sequence decreased considerably sooner and more quickly than in the case of those of direct sequence.

There is very close agreement between the pair of values just found and the number given above (p. 99, 1)—the procedure being, as here, without the exclusion of knowledge—for the learning of derived series which the day before had been learned in their original form to the point of first possible reproduction. This number, it is true, was obtained under somewhat different conditions. In the first place, not always were the same number of repetitions employed for learning, but each time as many as were required for the first possible reproduction—*i.e.*, not exactly, but on the average, 32. Moreover, the nature of the derivation of the series was somewhat different, as was stated above. But these differences have little weight in the case of numbers which otherwise could have little claim to exactness. I adduce therefore this value for comparison, and in addition the numbers given in Chapter VI for the influence of repetitions on the relearning of the same untransformed series. Here then is the table.

Number of Repetitions	Time for relearning the untransformed series after 24 hours	Time for relearning after 24 hours series transformed by skipping one syllable	Saving in relearning unchanged series	Saving in learning the changed series	Saving in changed series in % of the saving for the unchanged
0	1270				
16	1078	1170	192	100	52%
32	863	1121	407	149	37%
64	454	1109	816	161	20%

(The numbers of the four intermediate columns mean seconds.)

I call attention again to the fact that the numbers given above are in part rather inexact and that they were gained under very limited conditions. However, it is allowable to sketch summarily and with hypothetical elaboration the view which these results make appear to be the most probable explanation of an important group of inner processes and which fills pleasingly and completely a hitherto empty place in our knowledge.

With the imprinting and internal fixation of an idea-series through its manifold repetition, inner connections, associations, are woven between all the separate members of the series. The nature of these is such that series made out of members thus associated are picked up and reproduced more easily, with less resistance to be overcome, than similar series made up of members not previously associated. Their nature can also be stated in this way, that each member of the series has the definite tendency on its own return to consciousness to bring back others with it. These connections, or tendencies, are of different strength from several different points of view. For the more distant members of the original series they are weaker than for the nearer; for specific distances backwards they are weaker than for the same distances forward. The strength of all the connections increases as the number of repetitions increases. But the originally stronger threads between the nearer members are strengthened considerably more quickly than the weaker ones which connect the more distant terms. The more, therefore, the number of repetitions increases, the stronger, both absolutely and relatively, become the connections between immediately successive terms. To the same degree the more exclusive and dominant becomes the tendency of each term at its own return into consciousness to draw after itself that term which had always immediately followed it during the repetitions.

Section 42. Indirect Strengthening of Associations

I conclude with the mention of a noteworthy fact which appeared incidentally in connection with the investigations mentioned in the preceding paragraphs. On account of the uncertainty of the numerical results which come into consideration, I can call attention to it only with great reserve. I cannot, however, pass it by altogether because it is probable in itself, a

because, with further confirmation, it will throw a characteristic light on inner processes which are actually present but which remain unconscious. It will also reveal the relative independence of these processes from conscious accompaniments, as I have shown above (§ 24).

The derivation of the transformed series in the case of the last mentioned investigations was accomplished, as has been stated, in the following way. Out of two 16-syllable series selected by chance, first all the odd-numbered syllables were combined to form a new series and then all the even-numbered to form a second series which followed in immediate succession. In the case of a group consisting of six series of this sort, therefore, the derived series II contained nothing but syllables which in the first process of memorising had followed immediately upon the corresponding members of series I. The derived series IV bore a similar relation to series III, and series VI to series V. The following phenomenon appeared, which is the peculiar relation to which I wish to call attention. Less time was required for learning by heart series II, IV, VI on the average than for series I, III, V, although in all the other groups of series, whether original or derived, the converse was the case. I adduce some numerical data in evidence of this relation.

From all the experiments with six series of 16 syllables which were learned to the point of the first recital, ten immediately successive experiments are chosen by chance for two different time-periods. The times for committing to memory series I, III, V are combined in calculation, as are also those for series II, IV, VI.

1

A Sum of series (I, III, V)	B Sum of series (II, IV, VI)	Δ (B–A)
467	790	323
544	666	122
662	704	42
548	668	120
523	539	16
475	657	182
612	753	141
853	548	—305
637	641	4
499	780	281
m 582	675	93 P.E.$_m$=±37

2

488	694	206
604	704	100
551	734	183
596	637	41
559	686	127
611	744	133
653	682	129
598	700	102
723	606	—117
643	678	35
m 603	687	84 P.E.$_m$=±20

The sum of series II, IV and VI, found by averaging the ten experiments, is here in both cases, as can readily be seen, considerably greater than the sum of series I, III, V. The differences are, to be sure, of very different amounts for the separate experiments, and in one case they have a pronounced negative value; but these fluctuations are represented in the large probable error of the differences of the averages; and, in spite of the size of these errors, the positive character of the differences may be considered as fairly certain.

In all other investigated cases the following result appears: there are large fluctuations of the differences in the individual experiments, but a combination of the several experiments shows a decisive predominance for series II, IV, VI although the surplus is smaller than in the case of the two experiments in question. Thus in the case of 11 earlier tests in which series

were learned by heart which had been derived by skipping one intermediate syllable and which had been learned the day before in the original form the results were (p. 99, 1):

Sum of series (II, IV, VI) minus Sum of series (I, III, V) $= 33$ ($P.E._m = 23$).

With six later tests of the same sort (p. 103, 1):

Sum of (II, IV, VI) minus Sum of (I, III, V) $= 42$ ($P.E._m = 29$).

With ten experiments with series which had been repeated the day before 16 times each (p. 55):

Sum of (II, IV, VI) minus Sum of (I, III, V) $= 17$ ($P.E._m = 21$), etc.

On account of the largeness of the probable error a single one of the last given figures would have little significance. By means of their correspondence as to the nature of the difference they gain in probability, and the phenomenon becomes quite intelligible in light of the results of Section 18. There, and with especial clearness in the case of 16-syllable series, it was shown that the learning of the individual series occurred in the form of fairly regular oscillations. These were of such a sort that a relatively slowly learned series followed one learned relatively more quickly and vice versa (p. 43, Fig. 3). Since in the case of each experiment the first series was learned on the average the most quickly and the second the most slowly, by the combination of series I, III, V the average minima are united and of series II, IV, VI the average maxima. The difference, *S* (II, IV, VI) minus (I, III, V) is, therefore, in general positive.

Accordingly it must be surprising that in the case of both the groups of tests mentioned in the preceding paragraphs, this difference is on the contrary of a negative sign.

(1) The results in the case of learning derived series which had been repeated 16 times on the day previous in their original form were as follows:

A Sum of (I, III, V)	B Sum of (II, IV, VI)	Δ (B–A)
656	522	—134
702	514	—188
603	613	10
450	500	50
662	696	34
560	459	—101
588	603	15
637	593	—44
Av. 607	562	—45 $P.E._m$ ±21

(2) The results of learning derived series which had been repeated 64 times on the day previous in their original form were as follows:

A Sum of (I, III, V)	B Sum of (II, IV, VI)	Δ (B–A)
515	642	127
567	415	—152
626	572	—54
588	560	—28
543	452	—91
539	478	—61
584	599	15
592	604	12
Av. 569	540	—29 $P.E._m$ ±20

The fluctuations of the numbers for the separate experiments are also in this case very great. However, it is evident on the first glance and without further comparison that a strong displacement of the differences to the negative side has taken place. This fact is also expressed by the averages. In contrast with previous results, the series II, IV, VI were learned in somewhat shorter time than series I, III, V.

That this exception rests on mere chance is possible but not very probable. The probable errors, although large, are not large enough to indicate this.

I would sooner fear that it was a case of disturbance of the results through the oft-mentioned source of error, anticipation of the outcome (p. 27 ff. and p. 101). During the progress of the experiment I believed with increasing certainty that I could fore-

see the smaller expenditure of time for the learning of series II, IV, VI, and it was only because I thought something of this sort that I changed the method of derivation of the transformed series. I cannot, therefore, exclude the possibility that, merely on the basis of this hidden presupposition and in a manner altogether unrevealed to consciousness, a greater concentration of attention was present in learning series II, IV, VI than in learning series I, III, V. However, this assumption is not to be taken positively as the correct one. The assumption that the whole of the difference found is to be traced back to the influence of this source of error would involve the ascription of a pretty large function to an involuntary and completely unconscious accommodation of attention due to a secret expectation.

There remains, accordingly, a certain probability for the third possibility, namely, that the contrasting character of the average differences has in part at least an objective basis, that the more rapid learning of the derived series, II, IV, VI, was in part due to their manner of derivation.

The proper way in which to think of this causation would become clear only by the introduction of physiological conceptions which must first be constructed or at least remodelled. If use is made of the language of psychology, then, as in the case of all unconscious processes, expression can be only figurative and inexact.

As a result of the learning by heart of a series in the original form the separate syllables, we must say, retain fairly strong tendencies upon their own return to consciousness to bring after them the syllables which immediately succeeded them. If, therefore, the syllables 1, 3, 5, etc., return to consciousness, the syllables 2, 4, 6, etc., have a tendency also to appear. This tendency is not strong enough to bring about as a consciously perceivable event the actual appearance of 2, 4, 6. The latter are in evidence only in a certain inner condition of excitability; something takes place in them which would not have occurred if 1, 3, 5 had not been repeated. They behave like a forgotten name which one attempts to recollect. This is not consciously present; on the contrary, it is being sought. In a certain way, however, it is undeniably present. It is on the way to consciousness, as one might say. For if ideas of all sorts were called up which stood in connection with the earlier experienced

name, a person could usually tell whether they agreed with the one now sought for but not yet found, or not. As a result of the frequent repetition of the syllables 1, 3, 5 previously connected with the syllables 2, 4, 6, the latter were placed in a similar slightly pronounced condition of excitation, lying between conscious appearance on the one side and simple non-appearance on the other. And this excitation has, as it now appears from our tests, a result altogether similar to that of actual return to consciousness. Inner connections are established between successively and internally aroused syllables just as between syllables successively raised to consciousness, except that the former are naturally of less strength. Secret threads are spun which bind together the series 2, 4, 6, not yet aroused to consciousness, and prepare the way for its conscious appearance. Such threads existed already in greater strength as a result of the learning of the original series; the present effect is that of strengthening somewhat connections already made. And that is nothing else than what was found above: if two syllable-combinations—1, 3, 5 . . . and 2, 4, 6 . . . —are frequently associated in consciousness (the learning of the original series) then the subsequent learning of the second combination (derived series II, IV, VI) soon after the learning of the first (derived series I, III, V) has considerably less resistance to overcome than the latter. A certain strengthening of associations takes place, not only directly, through conscious repetition of the associated members, but also indirectly through the conscious repetition of other members with which the first had been frequently connected.

This way of viewing the matter is a consequence of the assumption (which became necessary above, p. 109) of the formation of associative connections over more intervening members than could be comprehended in one clearly conscious act. These connections would be very fruitful in the explanation of many surprising phenomena of memory and recollection, but on account of the uncertainty of their experiential basis I refrain for the present from pursuing them further.